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THE

JOURNAL OF SCIENCE.

OCTOBER, 1885.

I. THE INTERBOND OF THE SEEN AND THE UNSEEN.

Collated by the AUTHOR OF "SCIENTIFIC MATERIALISM."

The narrative is generally in the words, but always in the sense, of the enunciations of Andrew Jackson Davis, when in his Seer state. These utterances were taken down by Mr. Fishbough as they were delivered, and afterwards published by him in a work called "NATURE'S DIVINE REVELATIONS," *in three parts, viz., "The Principles of Nature," "Nature's Divine Revelations," and "A Voice to Mankind."*

"There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy."—*Hamlet.*

THE work "Nature's Divine Revelations" was delivered by Mr. Andrew Jackson Davis when in a state of clairvoyance induced by Mesmerism, the manipulator being Dr. Lyon Smith.

Before entering upon the subject of the work given under the peculiar condition termed *Clairvoyance*, it seems wise to give a synopsis of the history of the clairvoyant, by Mr. Fishbough (the reporter of the utterances as they were delivered). The short memoir here given is a digest of a biography appended to the work. Mr. Fishbough, its compiler, procured all evidences attainable by him of the childhood and youth of Mr. Davis up to the period of the

delivery of the lectures (as they are termed), evidenced by various letters from various persons who were thoroughly informed upon the subject. Of Mr. Fishbough's capability for the position assigned to him, and of his absolute integrity, there can be no possible doubt. Nor is this all: the lectures were attended by many persons; a list of the most prominent will be found in page 2 of the work. On the manuscripts of one hundred and forty (MSS. Reports) there are two hundred and sixty-seven signatures of persons who had been witnesses of their delivery. Here and there is a lecture which has no signature (seventeen in all), "but the identity of the source of these with that of the others will not be doubted by anyone on proper investigation."

The evidences authenticating the whole proceedings have been most carefully preserved; they are so complete as to leave no room for doubt. The lectures themselves, all circumstances considered, afford the greatest of the evidences.

Mr. Davis's father was a shoemaker in poor circumstances. Neither father nor mother were particularly inclined, if at all, to intellectual pursuits, and the son received, but for a very limited period, such education as could be acquired in a district school, what in England at that period would have been termed the Parish School; in the whole five months, wherein he learned to read imperfectly, to write fairly, and to do simple sums in arithmetic, and had no desire to study, or to improve himself. He is supposed to have been born on August 11th, 1826. In early boyhood he was employed as a cattle-keeper; afterwards, when twelve years old, for eighteen months he worked with his father at his trade: he then was engaged in a grocery store, where he continued until he was fifteen, when he was apprenticed to Mr. Armstrong, with whom he continued two years, who says—"His education barely amounted to a knowledge of reading, writing, and arithmetic. His reading was exceedingly limited, and confined to that of a light and juvenile description. During his two years apprenticeship he established a character for faithfulness and integrity not to be surpassed, and which is seldom equalled." Mr. Bartlet gives a somewhat different account. After A. J. D. had left Mr. Armstrong, he says, he knew him from 1842 to 1845; at an early age he was thrown on his own exertions for support. "At the date of our intimacy no one could resist the conviction that his advantages had not been sufficient to give him common proficiency in the simplest elements of the English language. Decided disadvantages were all around him . . . so far as obtaining an education was concerned; for during

my acquaintance with him, which was most intimate from A.D. 1842 to 1845, he had not an hour's schooling, nor could he have afforded it consistently with his circumstances, yet he possessed an enquiring mind,—loved books, especially controversial religious works." "The singular powers claimed for Mr. Davis were brought into public notice by Mr. Wm. Levingston, a tailor, by whom he continued to be mesmerised for eighteen months."

On December 1st, 1843, the first experiment of his clairvoyant state took place under various tests which were quite successful; the result was to establish his power of interior sight beyond dispute.

Mr. Fishbough became acquainted with him in July, 1844, at the time when Mr. Davis, a boy of 18, was thrown into the mesmeric state for the purpose of examining disease. Before him "the human system seemed entirely transparent, and to our utter astonishment he employed the technical terms of Anatomy, Physiology, and *Materia Medica* as familiarly as household words. . . . From infallible indications presented we saw that there could be no collusion or deception, and no such thing as receiving his impressions sympathetically from the mind of his magnetiser."

Mr. Fishbough is entirely of opinion that the work was not the result of sympathetic impulse. There were in all one hundred and fifty-seven lectures; the time of delivery varied from forty minutes to four hours, commencing November 28th, 1845, and concluding January 25th, 1847. Three witnesses, a mesmeriser, and a scribe were chosen by Mr. Davis when in an abnormal state. Other persons also were admitted during the delivery of the lectures, "ranging from one to six, whether they were believers or unbelievers in clairvoyance, and such persons were always instantly distinguished by the lecturer while in his superior state."

For other matter respecting the lectures, or Mr. Davis, reference must be made to Mr. Fishbough's memoir in the commencement of the book.

The work contains 782 pages 8vo., of close print. However peculiar its phraseology and immense its tautology, it professes to be a Philosophy, comprising Cosmology, the peopling the world, advent of man, and his destiny. There is also a complete Biblical commentary. It must be confessed that the reading the work is one of intense and wearisome labour, yet to the curious in such literature the labour is repaid, as it exposes a really grand philosophy. The key-note of the whole is—From *Motion came Matter*; from Life, with the addition of Motion, the Vegetable Kingdom arose;

from Motion and Life, with the addition of Sensation, the Animal Kingdom arose; from Motion, Life, and Sensation, with the addition of Intelligence, the Human Family: thence follows the Soul or spirituality of man. It is difficult to class the views of the lecturer: at one time they are Materialism, pure and absolute; at another time they appear to be wholly Spiritual. The eventual solution is the education of the soul in this world and in that to come, the consummation being Wisdom, Purity, and Truth, and the indwelling with the Divine mind, which is described to be the soul of the Great Positive mind, out of which all things flowed and in which all things are.

All theologies are commented on, and none are accepted, the only realities of the soul being Love, Wisdom, and Truth.*

The whole system is an undoubted philosophy, and one of a high order. There are of course many errors and crudities, and assumptive conceits; despite all the peculiarities, even when dissent is excited, the feeling is, to say the least, one of utter astonishment that such utterances can have proceeded from, in a worldly point of view, an ignorant and uninformed man; for it would tax the knowledge of many well-read men in various branches of Thought and Philosophy, even if the knowledge was combined, to produce the system in comment.

It is my purpose, without the expression of my own opinion, to give not a digest,—that the limits of the Journal would render impossible,—but to notice the general aspects of the work, and if possible to render a true but running account of the first two books, *viz.*, "The Principles of Nature" and "Nature's Divine Revelations." The "Voice to Mankind" is out of my province: those who delight in the most utter Socialism may probably find matter congenial to their minds.

The Key or "Principles of Nature."

"Reason is the principle belonging to man alone; the office of the mind is to investigate, search, and explore the principles of Nature, and trace physical manifestations in their many and varied ramifications. Thought in its proper nature is uncontrolled—unlimited." "Ignorance, Superstition, and Bigotry have wielded a tyrannical sceptre, and

* "Alas! when will men perceive the difference between religion and priestcraft? When will they perceive that reason, so far from extinguishing religion by its more gaudy light, sheds on it all its lustre?"—BULWER.

Sectarianism has usurped the dominion over the human soul." "Man has lost much of the light and knowledge which he would now possess if the *real* had existed in place of the artificial."

Institutions have been founded on artificial demands, to support which man, against his nature and duty, and at the entire sacrifice of justice, feelings, and conscience, has used the sword against his fellows. War, Persecution, Vice, and Misery have thus reigned: thus the Moral, Social, and Intellectual have been crushed and prostrated, and all the mental slavery forced upon man has been caused by a perversion of his natural inclinations and of the principles of Nature. The hope of ameliorisation must be based on mental progress, when examination of the experience of the errors of the past will serve as a monitor for the future, but in the present there is no liberty for the unrestricted exercise of thought and its expression.

Would it not be well to understand and recognise the law which controls Nature and man with an undeviating and unchangeable government, rather than rely upon the rule of artificial laws? Then the great tree, so long concealed from mental view, whose roots have existed eternally, founded only in that great system of Nature of which man is the subject and effect would grow and progress: its fruits would then be the united interests of mankind: then ignorance, which has been and still appears to be inexorable in its influence, would be destroyed by the annihilating hand of truth and wisdom.

Theories founded on visible facts men have laboured to reduce to Science, believing the fact was attributable to some cause invisible, search was made and the cause demonstrated. Science founded on this ground cannot be overthrown,—*i.e.*, the causes are true and the effects are true. But those who expect effects to manifest themselves to the senses as *unchangeable* would be mistaken as respects their *particulars* and *minutiæ*. There are such things as general causes to produce general effects; but there is not such a thing as a general cause manifesting invariably both *general* and *minute* effects, particularly when these effects are looked for or expected in *forms*.

"Reasoning from effects in a retrograde manner men have arrived at non-entity, and become confirmed in a disbelief of anything beyond what the senses will recognise as existing. Are you not convinced, by the fluctuating nature of things external, fluctuating, and transient, and which are fleeting

and evanescent as the leaves of trees, that these are mere *effects*, and not *realities*?—that the *real* or *cause* is *invisible*, and that the effect is a mere transient *expression*? Matter and form cannot be depended upon as producing the same and unchangeable appearances, for there is a perpetual exhalation and inhalation going on throughout all Nature imperceptibly—a steady flux and reflux of matter." The causes of these invisible changes are invisible, "not to be heard or seen, yet nevertheless actual and real." Thus "make the evidence of truth the amount of evidence received *through* the organisation, and not that originating in the organisation." The outward is an instrument, an agent, a medium through which may be tested things external. Thus "the *body* is a *form*, is *transient*, is *changeable*; the *internal* is *not* changeable. The *man* is the *internal*; the *effect* or *form* is *external*. The mind is not acted upon by, but acts on, the body. That which is internal is the reality; that which it acts on is visible and mortal." Tracing effects shows an incalculable amount of causes and effects. Tracing causes to effects and effects to causes is the true mode of reasoning, and this you do in imagination until is arrived at "a chaos of existence: there stop breathless, and return to ask—What was the cause of the *first* cause? You would not have gone on these trackless peregrinations if you had but considered all forms and externals are not *causes*, but *effects*."

The course of reasoning to be pursued will be "to commence at the First Cause, and trace causes to effects until they reach the human body, which is an ultimate effect of the Great Cause," and then inversely. The mode of reasoning will be understood by the following illustration:—Conceive a germ hidden in the Earth; forget its existence, yet in a few years cast your eyes on the spot, it is a tree: "it is as impossible to disbelieve its *existence* as to doubt the germ which produced this existence. The tree stands and is visible as an ultimate; man stands and is also an ultimate. The germ of the tree, you knew of its existence, but the germ of the world you do not know of its existence; but is it not evident that the *latter* is at least possible, since the first is known and demonstrated?"

"All things external in forms, series, and degrees, appear to different persons to present different manifestations." A note by Mr. Fishbough explains "that forms, series, and degrees is the unfolding of the various and successive stages or kingdoms of creation."

"You have a foundation of belief called the evidence of

the senses,—*e.g.*, vision. All give their united testimonies that they *see* the sun rising in the east and setting in the west. The unreality of this position is shown in that the Sun, as regards the Earth, does not move. It is the visible and external you are deceived by, and not the invisible, which is the reality." Thought is invisible; the only mode by which the world becomes convinced of its reality is its manifestation in the body. You see light as from fire; if you had not a previous conviction that it would burn, you could only know that it would do so by contact. "This presents evidence . . . that the internal is the *cause* and *reality*, the external the effect."

"It is now clearly seen and proved that the things which constitute *causes* are invisible, and that their *effects* are manifested in all forms external. Hence the latter cannot be *realities*, for such a supposition would involve a self-inconsistency."

Animal Magnetism.

"This science is founded on two fluids, *magnetism* and *electricity*:" then follows a description of the senses, vision, hearing, smelling, feeling. "Each of these senses may and does perform its office indirectly on the mind without any direct interference or connection with anything external." During sleep the system is recruiting itself, but the organs of sense are closed to external impressions; the mind then makes imaginative excursions to different places and contemplates different things in existence; it supposes it sees, hears, walks, tastes, and sometimes suffers excruciating pains; it also appears in many places where it had no previous desire or intention to be. This cannot be *Spirit*, but is *Mind* connected with the body, and its impressions are received through the disturbed condition of the nerves of sensation. "This proves that there is an *internal* medium of sensation by which the mind enjoys its capacity as if the external was in connection with the world."

"Having established the existence of an independent conscious medium, I now explain its existence, its foundation being the muscular and sympathetic nerves." Man has two coatings, the *serous* and *mucous* surfaces. "The *serous* covers each organ, nerve, and fascia of the muscles, including the whole of their surfaces; the *mucous* surfaces constituting the inner of every organ, nerve, and muscle. The serous surfaces and mucous generate and sustain a positive and negative fluid; this controls the circulation.

The negative expands the ventricles, and that attracts the blood to its reservoir. The positive contracts the ventricles, and thus repulses the blood throughout the system. Hence there is a continual expansion and attraction, and contraction and repulsion, which illustrate familiarly the office of these forces. The serous surfaces are susceptible of feeling, whilst the mucous surfaces are not. The muscular nerves are controlled by the mind, whilst the sympathetic, or nerves of sensation, are the medium of actuating the mind when motion produces thought."

"The brain is composed of a sensitive and complicated system of fibres, to which no other part of the body bears any analogy. Being sensitive, it is *attractive* or *positive* to all that is existing on the nervous medium: hence it receives impressions irresistibly. It possesses within itself the positive and negative poles, or greater and lesser parts,—the one controlling, the other subject; the one receiving power, the other transmitting and exercising power. The ethereal substance which serves as a medium may be termed *magnetism*. The muscular motion of the system is performed through the medium of the substance which may be termed *electricity*.* When there is a full and uninterrupted exercise of all the powers and organs of the body,—when there is harmony existing throughout the whole physical system,—there is perfect health and enjoyment, because its forces (which are positive and negative or magnetic and electric) are regularly performing their functions: this indicates a perfect condition of the magnetic or nervous medium." The body on being deranged shows a *loss* of the positive or negative power; when all are in unison the system is then thoroughly magnetised. In order to *demagnetise* it, this equilibrium must be overcome, and the positive power must be extracted by a power still more positive; and this will produce the unconscious state called the magnetic.†

* On this subject Dr. C. B. Radcliffe, M.D., F.R.C.P., has written a learned treatise ("Vital Motion a Mode of Physical Motion") in which he asserts the same position. Dr. Radcliffe's work is a scientific and practical illustration of S. T. Coleridge's idea, that electricity was the method of organised function ("Theory of Life"). West, of Alford, had some such idea, as had also Sir Chas. Bell and others. Humboldt believed in animal electricity.

† Dr. Esdaile, in India, employed mesmerism as an anæsthetic, as also do many physicians in North America. In 1856, at New York Hospital, I witnessed a most successful operation under mesmeric influence. An athletic man had his arm crushed so badly that it had to be removed at the shoulder-joint. The patient was placed under influence, and was carved at the discretion of the operators. I heard no sound, nor did I observe a muscle wince; I watched narrowly, because I was a disbeliever. The joint was disengaged from the socket, and the skin sewn over the wound. When all was completed the patient was awakened; he stared around him, but did not

"Magnetism composes the sphere—rather the atmosphere—by which every person is individually surrounded; besides there is a medium which extends through all things placing man over the lower animal creation. All animals are subjected to man's control by the positive or subduing power which he possesses, and they receive this to their minds by the same medium which exists between an organ and the brain. Man being positive. and all else negative, the latter must yield to his control."

When a system is *demagnetised* sensation then only exists upon the internal or mucous surfaces which produce vital action: the negative power only remaining, vital action becomes torpid and feeble, according with the loss of power which previously controlled it. This is the magnetic state; and in this peculiar condition the patient is in sympathy with, or is submissive to, the will or positive magnetic power of the operator. The subject and operator form *one* system in power. There is a visible disunion, yet there exists an invisible union. Thus the phenomenon does not controvert or subvert any of the laws of Nature, but should rather be looked on as a progression and further development of the laws which govern organic beings.

There are *four* magnetic states of magnetism. In the *first* no particular phenomena are witnessed: in this state the person loses none of his senses, but is subject to all external impressions. In the *second* state the patient manifests his intellectual faculties, but is deprived of all muscular power. In the *third* state there is a strong sympathy between the operator and his subject. The chain of sympathy which connects the mind of the operator with that of his subject is *animal electricity*—the fluid which is the agent of all muscular motion, and through which magnetic sleep is induced—this is supposed to be the *clairvoyant* state, but it is not; it stands in analogy to *natural somnambulism*. The first is a phenomenon induced by magnetism; the latter is a similar mental state naturally produced by an inactivity of the magnetic medium, or sensation. In the passage from the *third* to the *fourth* state the mind loses almost all sympathy which attaches it to the system, it being nearly disconnected, and sustains the connection by a very minute and rare medium, the same that connects one thought with

---

appear to manifest any sensation of pain. I forget the result. I have an indistinct idea that after a short time the man died, owing to a fever which supervened.

another. In the *fourth* state the mind becomes free from the organisation, except as connected by the medium as above: it is then capable of receiving impressions of foreign or proximate objects, according to the medium with which it becomes particularly associated. The body is at this time dormant and inactive in all its parts, except the negative, or muscular and vital action, which is constantly kept up and controlled by the united forces of the operator and the operatee. This stands in analogy with the state known as death.

"Death is produced by the loss of both forces; but the clairvoyant state is produced by the blending of the forces of the two persons, and making them physically equivalent to one. The mind, in the first case, loses all the medium which connects it with the body; the latter is the same state mentally with the former, with the exception of the mediums referred to." *

"All the phenomena are seen and do exist with every being. Their healthy state is the magnetic state; and the various conditions and development of the mind during sleep, until death, are analogous to the various phenomena induced by magnetism,—only one is an ordinary manifestation, and the other is a further development of the same principles and laws which constitute and govern animal organisation."

"A great question now presents itself for investigation. *Is there such a thing existing as independent clairvoyance?* I answer, There is. You ask for proof; such you shall receive by the following investigations."

The meaning to be applied to the word independent should be well understood. If it is understood as self-existent, abstract, indefinite, and without any connection, then it is nothing, "for in this sense there is no such thing as independent." But if it is understood as meaning "free from direct instigation, then it *has* meaning."

Speaking of himself in the superior state (fourth mesmeric state), he says:—"The medium existing between thought and thought, between mind and mind, between time and eternity, is the only active pervading medium which I am dependent on for the conception of thought, and for the perception of all things of a refined, ethereal, or spiritual constitution. This is whilst the activity of the body and mind

* Should the reader be interested to trace the above matter respecting the mesmeric states *in extenso*, he will find it set forth from pages 30 to 38, "Principles of Nature," the first part of "Nature's Divine Revelations," and it is surmised that the illustrations, &c., will well repay his industry.

appears to be sympathetic or dependent. I am not impulsed or impressed by the thoughts or feelings of a foreign person, though I am cognizant of them through the medium above termed ethereal."

(To be continued).

II. ON CERTAIN ERRORS IN LUNAR OBSERVATIONS HITHERTO UNKNOWN.

By Major-General A. W. Drayson, late R.A., F.R.A.S.

BEFORE chronometers had been constructed so as to give an exact measure of time, the Lunar was the best means of obtaining a close approximation to Greenwich mean time, when the observer was in localities distant from Greenwich. It is possible that an accident may happen to a chronometer, and no accurate means other than a Lunar can be made use of to obtain Greenwich time. Observing the eclipses of Jupiter's satellites may give an approximation to Greenwich time; but this is not so accurate a method as the Lunar, unless the observer possess a large telescope, and this telescope is fixed on a stand. No navigator ought to be unacquainted with the method of measuring the altitudes, and lunar distances, and of working out the calculations.

As a problem only, the Lunar is worth studying, as it presents some very neat and beautiful examples in Geometry, and affords an excellent test of the skill of an observer, in measuring altitudes and distances.

I believe I can claim to have measured and calculated more Lunars than most observers, because, during the thirteen years that I had the superintendence of the Woolwich Observatory, I instructed more than one hundred officers and many more cadets on this subject, and during some years I measured and worked out between three and four hundred lunars per year.

Having the means of knowing Greenwich time to a second,

I could test the accuracy of my observations by comparing the time found by the Lunar with the time shown by my chronometer. From these comparisons I discovered that, whilst under some conditions I obtained results which were correct to within five seconds of time, under different conditions I obtained results which were twenty seconds or more in error.

Having used the same sextants in each case, and these sextants being in perfect adjustment, I at first supposed that on some nights my eye was better suited for observation than it was on other nights, and that "personal error" was the explanation of erroneous results.

I found, however, that when I had tabulated these errors, there appeared to be a law connected with them, so I set to work to discover, if possible, some other solution of the mystery than that of personal error.

On looking over the article on Lunars, in an early edition of the "Encyclopedia Britannica," I saw it stated that some error existed, but where this error lay was not known. But the more perfect the instrument, and the more accurate the observers, the more certain was the error to be manifested. To search for and find the solution of a paradox is always interesting, and although the Lunar observation and Geometry generally are now out of fashion, whilst vague theories and speculations are popular, I yet trust that some readers may take an interest in a novelty, though this is a geometrical one.

I will first endeavour to explain the principle of the Lunar.

The Moon moves by its own independent motion from west to east round the Earth, its rate of movement being a little more than 13° in the twenty-four hours. If, then, the Moon was coincident with a star at (say) 9 p.m. on a given night, and moved directly away from this star, then on the following night the Moon at 9 p.m. would be slightly more than 13° to the east of this star. The star being fixed, whilst the Moon moves, causes the Moon to act like the hand of a clock, whilst the star remains fixed like a mark on a disc.

By measuring and calculating the exact distance of the Moon, from the Sun or from certain stars, we are provided with one item; the other item being—knowing what the exact distance of the Moon is from the Sun, or star, at certain instants of Greenwich time.

Now the distance of the Moon from the Sun, or from certain stars, is given in the "Nautical Almanac," for

intervals of three hours for every day of the year; consequently we can compare our measured distance with the calculated distance given in the "Nautical Almanac," and by proportion find what the Greenwich time was when we measured and found our lunar distance.

The practical method of making the observations is as follows:—

1st. When three observers can be obtained, one observer measures the altitude of the Moon; another, the altitude of the Sun, or a star; the third measures the angular distance between the Sun and the Moon, or between a star and the Moon.

2nd. When one observer only is available, he measures first the altitude of the body which changes its altitude more slowly, then the altitude of the other body, then the lunar distance, then the altitude of the body moving most quickly, lastly the altitude of the body moving most slowly. The times at which each altitude is taken, and the time at which the lunar distance is measured, and the change in altitude of the two bodies, can then be reduced to the time at which the lunar distance is mentioned, so that the same results are obtained by one observer as could be obtained by three, *viz.*, the exact altitudes of the Moon and star at the instant at which the lunar distance was measured.

The problem now before us is to obtain the exact distance of the Moon from the Sun or a star. The measured distance is not the true distance, because the star, in consequence of refraction, appears higher in the heavens than it is in reality. The Moon from the same cause appears higher, but the Moon in consequence of parallax appears lower than she is —consequently a correction is required to be made to the Moon's measured altitudes for refraction and parallax, the first being a minus, the second a plus correction.

There are several formulæ for working out a Lunar, and obtaining the true lunar distance from the measured lunar and altitudes; but I always found that students understood the problem better if I treated it as a pure matter of spherical trigonometry,—consequently I adopted in instruction the following method:—

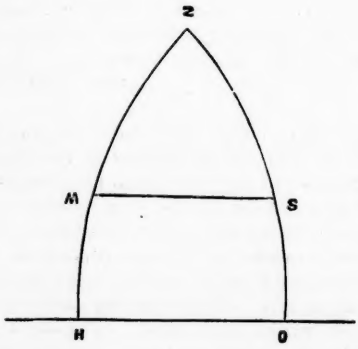
Having obtained the altitude of the Moon and star, as mentioned, and the lunar distance, do not correct for parallax or refraction, but find the angle with the Zenith, formed by the Moon and Star, as follows:—*viz.*, H M the altitude of the Moon as measured, consequently *Z* M the Zenith distance of the Moon.

O S the measured altitude of the Star, consequently *Z* S

the apparent Zenith distance of the Star. M S the angular measured distance between the Star and the centre of the Moon.

We have three sides—*viz.*, Z M, Z S, and M S—to find the angle at Z.

Having found the angle at Z, correct the Zenith distance Z M for parallax and refraction, and the side Z S for refraction.



Both parallax and refraction will be manifested in the direction of the vertical lines Z M and Z S, and will not therefore affect the angle at Z.

Then with the two sides Z M and Z S *corrected*, and the included angle at Z, find the value of the third side, which will be greater or less than M S, according to the position of the Moon and Star, and the third side, so found, will be the true distance of the Moon from the Star at the instant of observation.

By comparing the true distance thus found with the distance given in the "Nautical Almanac" for a certain time, we obtain the Greenwich mean time of the observation.

I have always found this method of teaching Lunars more readily understood than if I employed a sort of blind formula,—such, for example, as the following:—

M the apparent, *m* the true } altitudes of the Moon.
S the apparent, *s* the true } altitude of the Sun or Star.
D the measured distance.

Make $b = \frac{1}{2}(D+M+S)$.

$$\text{Sin } a = \frac{\left\{\text{Cos } b + \cos(b=D)\ \frac{\text{Cos } m\ \cos s}{\text{Cos M}\ \cos S}\right\}^{\frac{1}{2}}}{\text{Cos } \frac{1}{2}\{m+s)}$$

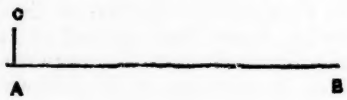
$$\text{Sin } \tfrac{1}{2}x = \text{Cos } \tfrac{1}{2}\{m+s\}\ \cos a.$$

x = true lunar distance.

We have now gone as far as what we may term the conventional Lunar, and yet there is an error which becomes apparent, when the most accurate instruments and observers have been employed. I will now point out where this error is.

The lunar distances given in the "Nautical Almanac" are calculated from the Right Ascension and Declination of the Moon and Star into each three hours of Greenwich mean time. These distances are the actual true distances of these bodies measured on the sphere of the heavens. The declination of either body enables the polar distances to be obtained, and the difference of right ascension gives the angle at the Pole. Consequently, with two sides and the included angle, the third side can be calculated. The error therefore is not to be found in this department, but in one of a far more delicate kind. In order to point out where the error exists, I must refer to the sextant itself.

When measuring angles between any two bodies with the sextant, the body which is looked at through the object-glass of the sextant becomes for the time being the pole of a sphere. If I set my sextant to any given angle, say 25°, and direct my sight through the object-glass to any particular star, I shall, as I turn my sextant round, cause each object 25° from this star to successively come in contact with the star, and all lines drawn from these objects to the star will be straight lines.



If I take two points on the horizon as follows, *viz.* A and B, 25° apart, and a third point (C) a few degrees vertically above A, the angular distance, B C, measured with the sextant will be greater than B A measured with the sextant,

because with B as a centre, and radius B A, the arc would pass on the B side of C.

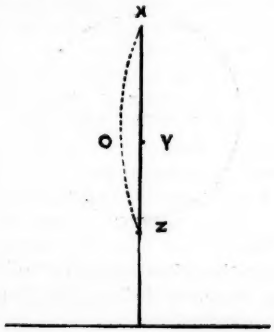
Every surveyor who has used the sextant knows that if he measures a series of angles round the horizon, and adds together these angles, their sum will amount to more than 360° if the various objects are not on the same level, as shown by C and B. This is due to the fact that C B is the hypothenuse of a right-angled triangle, the right angle being at A. Consequently B C measured with the sextant is greater than C A measured with the sextant.

Suppose we keep the three points C A B in exactly the same relative position as regards each other, but take the points A and B as each 30° above the horizon, whilst C is a few degrees vertically above A, and A and C both on the meridian. We have the same apparent conditions as before: C A B appears as a right angle, and the side C B represents the hypothenuse. Consequently, if we measure B C with the sextant, we should find this arc greater than A B; the fact of having elevated this triangle above the horizon, causing apparently no difference in the angular distance as measured with the sextant.

Now let us examine the contradiction which exists. Suppose A and B each 30° above the horizon, whilst C is on the meridian and a few degrees above A. The points C and B may, under these conditions, be both on the Equinoctial, whilst A is a point on the sphere, on the same meridian as C, and is south of the Equinoctial. It follows therefore that, calculated on the sphere of the heavens, the angle at C is the right angle, whilst the angle at A is less than a right angle. Consequently B A is the hypothenuse of the right-angled spherical triangle A C B, and not B A.

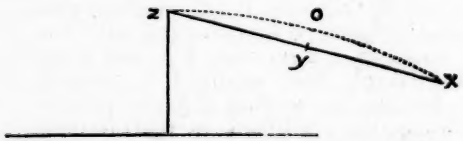
By looking through the object-glass of the sextant at B, we make B the pole of our sphere; and the straight line joining B and A, as measured with the sextant, is measured in the same manner as we should measure on a meridian, as shown below. Suppose B A a portion of the meridian, and B the pole, B A C a right angle; the angular distance B C, measured with the sextant, would be greater than B A, and if we turned this triangle down so that B A coincided with the horizon, or so that B A was parallel to the horizon, the conditions as regards measurements with the sextant would not be altered.

Again, suppose we have three stars vertically above one another, as shown in the following diagram,—*viz.*, *x*, *y*, *z*,—and these three stars are located on the meridian, the angular distance *x y* measured with the sextant, plus the angular distance *y z* measured with the sextant, will be equal to *x z* measured with the sextant.



Take *o*, a point about 2° to the left of *y*, the points *x o z* are not in a straight line, and the angular distance *x o*, plus the angular distance *o z*, measured with the sextant, will be greater than the angular distance *x z* measured with the sextant, two sides of the triangle being greater than the third.

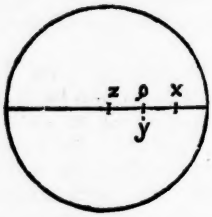
Now, as regards sextant measurements, these results would be the same, no matter how we moved these points, so long as they retained their exact relative position. Consequently we will keep the point *z* fixed, and rotate the other points round *z*, when they may be shown as seen below. As before, $z y + y x = z x$, $z o + o x$ will be greater than $z x$. Consequently $o x$ must be greater than $y x$, and $z o$ greater than $z y$, when measured with the sextant.



The point *z* is on the meridian, and the apparently curved line *z o x* represents the apparent curve of the Equinoctial;

but this curve is *on the sphere* a straight line, and if we represented these four points on the sphere they would be shown as in the diagram below.

The angular distance $z y$ calculated on the sphere, plus the angular distance $y x$ calculated on the sphere, will be greater than the arc of the Equinoctial $z x$.



Angular distances therefore measured with the sextant on the sphere of the heavens do not, in all cases, correspond to the angular distances calculated on the sphere, and which distances are obtained by the polar distances and difference in right ascension of two bodies.

The true angular distance of the Moon from a star, as given in the "Nautical Almanac," is calculated from the Polar distance of these two bodies, and their difference in right ascension. The measurements taken with the sextant, however, measures as we may term it *straight* between these two objects, and in some instances will give a less distance than that obtained by calculation.

Any person may test this problem by aid of a pair of compasses and a common globe, in the following manner:—Suppose you wished to travel from lat. 51°, long. 0°, to lat. 51°, long. 60° W.

Take a pair of compasses and measure along a parallel of latitude, on the Globe, a distance of 30° of longitude. Double this distance, and the point will be reached which has a longitude of 60° on the same parallel. Then measure the same half distances north of the middle distance, and with the compasses we shall reach a point west of 60° longitude, and on the same parallel. It is in fact the old story of great circle-sailing repeated.

When we measure angles with the sextant between two objects, we make the object we look at through the horizon glass the pole of our sphere, and a *straight* line from this pole to the second object is the angular distance measured.

The arc of the Equinoctial does not appear as a straight line when seen from middle latitudes, but as a curve. Hence measurements between two objects, when taken with the sextant, do not under all conditions agree with angular distances calculated in the sphere.

As I usually selected for the Lunar a time when the Moon or star was near the Meridian, so that its change of altitude in a given time was very slight, I found the error in the results which I have named, and the cause I have now described.

In the "Journal of Science" for July, 1874, and for January, 1875, I described certain facts relative to straight lines on the sphere appearing as curves to individuals on Earth. These facts, however, did not seem to be comprehended at the time, as most puerile criticism and arguments were urged against the facts, or attempted as explanations. The cause hitherto considered inexplicable is due to the same cause which I then described, and accounts for the errors in Lunars which every observer must have found do occur.

The problem, as a problem only, is interesting; but as it is a geometrical question, and Geometry is now too often ignored, and vague theories substituted for it, the Lunar observation may not be so well understood or as much practised as it was in former times.

III. MADMEN AND MADNESS AT ROME.

INSANITY forces itself upon public attention under several points of view. Its treatment, of course, belongs exclusively to the medical profession. So does its recognition, with a view to the restraint of these unfortunates, —a question which is for the present in a most unsatisfactory position. But the late serious increase of mental derangement, and the causes to which this increase is due, concern not only physicians and sanitary reformers but the entire public, and especially all persons who in any manner come forward as the guides and the teachers of their fellows.

The journalist, the preacher, and the statesman cannot afford to neglect a phenomenon so menacing. For, in spite of all the advantages which we enjoy as compared with our forefathers some centuries ago, insanity is increasing among us and among all so-called civilised nations.

In seeking for the cause of this alarming change for the worse the inductive logic bids us fix our attention upon those social factors which have been notably intensified during the last century or half century. Such factors are not few nor trifling, as a comparison between the years 1785 or even 1835 and the present 1885 will at once show. But upon which of these must the blame chiefly rest? In considering these questions it may be not uninteresting to examine the report published not long ago by Dr. Fiordispini, Director of the Manicomio,—the great asylum of Rome. This research comprises the seven years from 1874 to 1880. He insists at the outset that insanity is a disease which from day to day demands more numerous victims. It was once proposed to Pius IX. to build new barracks in order to keep down the Roman populace. The Pope replied in words, the truth of which becomes daily more striking:—"The Romans do not need new barracks, but a new lunatic asylum." Dr. Fiordispini adds that if things continue as at present it will not require a prophet to foretell that a new Manicomio will soon become necessary.

According to the official statistics of 1880, in the three years from 1877 to 1880 the number of the insane increased in Liguria by 23·15 per cent, in Umbria by 24·12 per cent, in Venice by 28·73, in Sicily by 43·88, and in Rome by 32·71. If we take into account the increase of population introduced into Rome by its being made the capital of united Italy we find that whilst there were in 1874 out of every 10,000 inhabitants 2·06 insane persons, and in 1877 3·35, in 1880, from the cessation of certain temporary causes, the proportion has fallen to 3·11.

What are the causes of an increase of which modern civilisation cannot be very proud, and which are certainly a grave set-off against whatever benefits there may have accrued to the Italian nation from its unity and its "redemption?" Dr. Fiordispini distinguishes firstly the general causes, common more or less to all Europe, and the local causes, peculiar to Rome, or at least to Italy.

As regards the former, he denounces what Bossuet calls the tendencies and the spirit of the age. He points to the demand for unchecked liberty in everything and for all, without distinction of age, sex, or condition; to the feverish

desire for the riches necessary for the gratification of new wants; the thirst for pleasure which has taken possession of the whole of society, directing its aspirations, stimulating its appetites, and causing it to subordinate everything to immediate material enjoyment. Now this positivism, this naturalism of life, this over-excitement of all the instincts, this "superheated" existence, can but lead to madness and suicide.

It will be of course remembered that suicide is increasing in the modern world no less decidedly than insanity. But leaving this fact on one side we may turn to the words "this positivism, this naturalism of life." Here, surely, are two words not well applied. Modern life seems to us to have at any rate a most decided tendency to unnaturalism, if such a word may be used. But this also is not our present subject.

To these general causes must be added others more or less peculiar to Rome, and here we follow verbally Dr. Fiordispini:—

"The Roman people passed suddenly in 1870, and without any gradual stages of transition, from the practice of confession to liberty of conscience; from the exposition of the Gospel and from Lent sermons to free conferences and public meetings; from religious marriages to civil unions; from clandestine prostitution to public, authorised prostitution; from a theatre subject to censorship to a theatre without rule or restraint; from family life to public life; from the aristocratic distinction of classes to their medley and confusion; from instruction directed by the Church to an instruction recognising no orders; from the exclusion of public affairs to the election of representatives; from a Pope-king to a constitutional government; and we shall easily comprehend how violent must have been the shock to the population and what disturbances must have been introduced both in the physical and the moral order."

It is of course not our province to decide which of these changes is for the better and which for the worse. But we cannot deny the extent and the abruptness of the alteration.

Rome does not in this respect represent an isolated fact. It is well known in France that the times of revolution are those which yield the heaviest contingent to the mad-houses. Buenos Ayres has furnished a sad instance of the same fact during the dictatorship of the notorious Rosas, and the recent events during the supremacy of the Commune at Paris are far from admitting of a contrary interpretation.

From the tables given at the conclusion of Dr. Fiordispini's

work it appears that at Rome for every 100 insane men there are only 58 insane women. The reason of this difference is that at Rome, thanks to the education which they receive, the women observe greater discretion, and especially avoid that alcoholism which is one of the principal causes of madness.

Dr. Fiordispini gives a table of the proportion of insanity in different nations. From this we learn that Italy as a whole is still in a better position than England. We have 1 insane person to 514; Italy has only 1 in 1000. But in a table given in 1857 in the "Alliance Prize Esssay," by Dr. F. R. Lees, the proportion of lunatics in Italy is given as only 1 in 3785. Consequently since that date (or from a somewhat earlier one, since the statistics given by Dr. Lees may have been collected a few years earlier) insanity has increased in nearly a four-fold ratio. Our position has also retrograded but in a less proportion, since in 1857 we had 1 insane subject in every 713 persons.

There is one point alike in the tables of Dr. Lees and in those of Dr. Fiordispini which is perplexing. According to the latter authority Belgium has the smallest proportion of insane persons of all the countries which he mentions, namely 1 in 1014. Dr. Lees gives the proportion as 1 in 1046. Now the circumstances prevalent in Belgium are those which are generally considered favourable to mental derangement Its consumption of alcohol per head is equal to that of England; its sexual immorality has been of late years painfully forced upon our notice. It has contested elections and even riots between Catholics and "Liberals," and lastly it is steeped to the very lips in commercialism, greed, and competition. Yet with all these circumstances it has a smaller proportion of deranged persons than any nation in Europe if we except Spain. This is a riddle to which we invite attention.

Returning to Dr. Fiordispini's report:—He classifies the causes of insanity as organic or physical, and as moral, the former being by far the more powerful.

In the first place he ranks alcoholism. The number of persons who have drowned their reason in wine (used here as a generic term), according to an expression too cruelly true, has been increased more than three-fold since 1873, and it is to be believed that it will go on augmenting. Not merely Rome but all other countries present an analogous increase of alcoholism. Up to 1870 Rome had lagged in the rear of the *civilising movement* of what is commonly known as progress.

Under the Government of the Popes there were certainly

drunkards, but these were protected against excess by prohibitive laws which touched the vendor, and restrained the facility for intoxication. The Roman, further drank no spirits, and the wines supplied to him cost the producer so little that adulteration was scarcely worth the trouble. To-day the sales of intoxicating liquors increase in a frightful proportion; the wine is sophisticated and is consequently the worse for health. But the most deplorable fact is that the people intoxicate themselves with spirits, the consequences of which are far more disastrous for the human economy. Dr. Fiordispini, taking his stand on the facility which the Roman people now possess for indulging their desire for intoxication, thinks that Rome will be soon in this respect on a level with the other capitals of Europe.

We are compelled here to draw a little closer attention to the change which has lately taken place in the quality of alcoholic drinks. The consumption of wines has decreased, while that of malt-liquors and spirits has increased. The distilled liquors of the day are no longer obtained solely from fruits, from cane-sugar (we do not apply this honourable name to betose), nor even from grain, but to a large extent from roots, and are rich in amyl compounds, commonly known as fusel. Now amylic alcohol has a far more deleterious action upon the brain than has ethylic alcohol. The introduction of the liqueur *absinthe*, now so widely used, has had a very alarming effect. It is to be deplored that our temperance reformers and our sanitarians never seem to seek for a total prohibition of the manufacture or sale of this poison in England, or its importation from abroad.

As regards wines they have long been fortified for the benefit of persons who judge a wine by its intoxicating power rather than by its flavour and aroma. But now the fortification is effected with potato-spirit rich in fusel, and there are even wines which contain not a drop of the juice of the grape. We need therefore scarcely wonder if the use of such wines brings on results which were formerly unknown in the wine-growing districts of Europe. The phylloxera has been a great demoralising agent.

The next physical cause of insanity is heredity, one of those whose effects are least open to discussion. Trelat pronounces it the *causa causarum* of madness. Sad that our law makers who strain at such a gnat as the marriage of a man with the sister of his deceased wife bolt such an albufaki camel as the intermarriage of persons belonging to families tainted with insanity. How much wiser were the laws of the ancient Buddhists? The next cause is a

generally dissipated life, the effects of which, however, are not easy to estimate, since it almost invariably includes alcoholism.

Curiously enough we find no remarks on the influence of consanguineous marriages as a physical factor in the production of insanity. Perhaps, however, the wise old law of the Catholic Church which denounced such marriages as incestuous is still observed in Italy, and thus Dr. Fiordispini may have had little opportunity of studying this part of the question.

According to many physicians moral causes alone can never engender insanity, and where it appears either heredity or some other physical cause must have been at work. Mantegazza even said that with a well constituted brain a man can never become insane.

The question arises, however, in how far moral causes acting upon the parent may be translated into physical causes—*i.e.*, defective or otherwise morbid structure of the brain—in the child or the grandchild?

Among the moral causes which Dr. Fiordispini discusses is contagion or imitation. It is impossible, he holds, to witness for any length of time the spectacle of the intellectual aberrations, the delirium, and the extravagant convulsions of the insane without undergoing a painful and a more or less durable impression upon the mind. This is an effect analogous to that produced by reading sensational novels, accounts in detail of the deeds and the trials of criminals, or by witnessing a certain class of dramatic performances. The readers or hearers of such productions are led by degrees to repeat actions identical with or analogous to those the impression of which is deeply engraven on their minds. At the Manicomio at Rome among a staff of 327 persons engaged in watching and attending to the insane, 3·98 per cent have been attacked with the same affection. This proportion is serious, as it amounts practically to 1 case of mental disease out of every 25 of these persons, whilst the proportion of the insane among the entire population of Rome is only 1 in 585. Hence in Rome the keepers and nurses in a lunatic asylum are 23 times more liable to become insane than are their fellow citizens, other things being of course equal!

We may here refer to Dr. Rambosson's interesting researches on the transmission of moral phenomena by contagion, and to the opinion expressed by Dr. B. W. Richardson that the insane exhale an essence of madness which may infect other persons by whom it is inhaled.

The action of competition and overpressure as most effectual moral causes of insanity, as well as of other diseases, is indisputable. But what of that? Whilst touching on this subject we feel bound to borrow the following just remarks from the "Medical Press and Circular." Says our contemporary:—

"It is not often that an ostensibly respectable journal has to sup humiliation, publicly and copiously, as the "Spectator" had to do on Saturday last, when it was compelled to publish a letter from Dr. Crichton-Browne convicting it of having knowingly said what was directly untrue in a review of a Danish work on Overpressure. The attitude which the "Spectator" has adopted and maintained towards the medical profession throughout the overpressure controversy has not in any degree surprised us, for it might have been expected that a journal that gushes over the twitchings of a decapitated frog would be deaf to the cries of suffering children; but the discreditable position into which its rancour has betrayed it does certainly awaken wonder as well as commiseration. It was scarcely to have been anticipated that a highly superior journal that is for ever congratulating itself that it is not as other journals are should have been caught fibbing flagrantly, and has been ignominiously punished on the spot. Yet such is unhappily the case; and so audacious is the untruth that has been brought home to our contemporary that it is almost impossible to avoid the conclusion that this is not its first offence. Presuming, we suppose, on the indisposition of authors to meddle with their critics, even when they are very far wrong, it repeated the misrepresentation which was put in circulation last year, and was then contradicted, that Dr. Crichton-Browne's report on 'Overpressure in Schools,' was an officious and spontaneous production which was violently thrust upon the mild and unsuspecting Mundella, very much against his will. But it miscalculated the endurance of Dr. Crichton-Browne, who has promptly turned upon it and compelled it to eat its leek with what grace it could muster. With an extract from 'Hansard' containing Mr. Mundella's intimation to the House of Commons that he had invited Dr. Crichton-Browne to report to him, held up before it, it admits that it was 'mistaken.' But this is not a question of mistake, but of mendacity, for the accusation is that the reviewer made his untrue statement knowing the real facts, and no one can question that the charge has been fully proved. Our contemporary is abject, but we fear unreformed, for in its note to Dr. Crichton-Browne's scathing letter, it performs a

wriggling movement which might be called mendacious. Endeavouring to show that if it was wrong on one point, it was right on another, it argues that the report referred to was really unchecked, because Mr. Fitch's little memorandum was not written until after it was finished, thus ignoring the fact mentioned in Dr. Crichton-Browne's letter that Mr. Fitch accompanied him on some of his visits to schools, and had opportunities of checking his observations while they were being made."

IV. SANITARY AGENCY OF LIGHT: INFLUENCE OF THE SUN UPON THE VITALITY OF MICROCOCCI.

THAT light is essential to a vigorous state of health has long been admitted; but that the sun is a "sanitary" agent in the narrower sense of the term, a powerful ally in our war against so-called zymotic diseases, is not fully known: at least it has not yet met with practical recognition.

M. E. Duclaux, some little time ago, laid before the French Academy of Sciences a series of observations on the influence of sunlight upon disease-bacilli. He has now extended his researches to micrococci. Of these minute beings he has specially examined six kinds, including those of *Pemphigus*, of *Impetigo contagiosa*, one found in the blood and urine of a patient suffering from rheumatic nodosities, &c. All these micrococci show a considerable resemblance both morphological and physiological, but they are nevertheless specifically different.

The action of the sun upon them differs accordingly as it is exerted upon an individual more or less mature, and whether it has been preserved in the dry state or in a culture-fluid. It varies, also, according to the season of the year. As regards this last factor M. Duclaux has not sought to eliminate the action of solar heat from the influence of light. This may, from one point of view, be regarded as a defect

in his observations. But he—in our opinion very judiciously—thought it well to remain in the normal conditions to which microbic germs are exposed when suspended in the air or deposited on the surfaces of various bodies. It is to be hoped, nevertheless, that a second series of researches may soon follow, to ascertain the influence upon these beings of light alone, as distinguished from heat. The only condition which has been observed as regards heat was not to exceed the temperatures which have been found most suitable for their cultivation, and which lie between 86° and 104° F. The temperature in the sun sometimes rose much higher. The limits of vitality which M. Duclaux found in his experiments are therefore maximum limits, and are so especially under the influence of the solar light.

The results may be briefly summarised as follows:—Young cultures of the micrococci in neutralised veal broth, which live on an average more than a year if kept in the dark or in diffused light, only resisted this year the feeble and intermittent sunshine of the spring for forty days,—that is, from May 4th to June 13th. In July fifteen days of exposure were sufficient to kill them, and a shorter time sufficed to attenuate them and to deprive them of all action upon the organism even of those animals which are most susceptible to their influence.

The vitality is still feebler if a micrococcus is preserved in the dry state, in the imperceptible coating which is left by a drop of culture liquid evaporated on the bottom of a flask. The micrococcus of the "Biskratt boil" and that of *Pemphigus*, which were found still living after being kept for five to six months in this dry condition in the dark, have been killed by eight days' exposure to the sun, from May 26th to June 3rd. In July two to three days were sufficient, and it was about the same for other species. None of those which M. Duclaux studied resisted three days of insolation, from July 7th to 9th, at a window facing the south, but which received the sun only for four hours daily, from 9 a.m. to 1 p.m., and where the temperature never exceeded 102° F.

These are lower figures than those which the same biologist has previously found for bacilli. The difference probably turns on the fact that in the micrococci the most resistant state of these beings, if it exists at all, is rare and still unknown.

But however this may be, it is interesting to establish the fact that a few hours of exposure to the sun suffice first to attenuate and finally to kill these micrococci, which include so many formidable and widely distributed species. It thus

becomes intelligible why the air carries along so many dead germs; why many diseases, in spite of winds and atmospheric currents, remain confined in their places of origin, and that, when coming from afar, aërial germs reach us in an attenuated condition.

To retain their virulence they require to be conveyed in bales of merchandise, in clothing packed up, or in the dark and moist holds of ships.

In fine, M. Duclaux pronounces solar light the most universal, the most economical, and the most efficient agent which we can employ, either in private or public sanitation.

We may be permitted to point out certain practical applications of the truths which experiment has thus brought to light.

It is generally admitted by official, or would-be official, sanitarians that one of the greatest evils of polluted waters is that they serve as a nursery for the propagation of disease-germs, and as an organised service for their distribution. Surely, therefore, it is exceedingly important that the light of the sun should penetrate, without let or hindrance, to and into such waters. But so long as any water is rendered turbid by suspended matters, or deeply coloured by waste waters from tanneries, dye-works, &c., the germ-destroying action of the sun upon such waters is much enfeebled or altogether annulled.

We have therefore to ask, what is the meaning of the sneer that "clarification is not purification"? Be it so; but if clarification permits the sun's rays to penetrate into waters where they were previously shut out by the opacity of suspended filth, clarification is a step—and no unimportant step—towards purification.

Again, we have heard municipal authorities contend that the removal of, or the exclusion of, colouring matters from a river is of no consequence. "So long as we are free from stench," they argue, "the colour of the river is of no consequence." We now see that it is of great consequence, not merely from an æsthetic or sentimental, but from a practical and sanitary point of view.

This is a consideration which tells heavily against precipitation processes in which lime plays an important part. We know a town containing extensive tanneries and dye-works. We know that its sewage can be readily decolourised by salts of alumina, whilst if treated with lime-water or cream of lime it takes an intense mahogany colour, admirably adapted to hinder the sun's rays from penetrating.

Surely we may say that without clarification and decolourisation there can be no thorough purification!

But we have now to call to mind the fact that certain systems of sewage treatment—systems which have been loudly vaunted, and which it has even been sought to force upon the nation to the exclusion of all others—actually withdraw the sewage and waste waters from the action of light. Take, for instance, "broad irrigation." The sewage, conveyed from the town down to the irrigation farm, rapidly finds its way into the earth, where it is protected from the sun. We may grant that the lifeless organic matter, present in the sewage in suspension or solution, will be to a great extent oxidised or burnt up. But there is no evidence that this fate awaits the living micro-organisms of the sewage. On the contrary, there is positive evidence—as in the celebrated Swiss case—that they may and do emerge from the earth without being attenuated. Further, in a well-arranged precipitation system, the water—rendered transparent by the removal of colouring-matter and suspended impurities—is exposed for some time to the light in the precipitation-tanks, and after that it is often conveyed in open shallow channels for some distance, having the fullest opportunity of insolation.

It is, perhaps, rather a misfortune that other considerations compel us to convey sewage from the places of its production in covered channels, and thus seclude it from the beneficent action of light.

There are quite other spheres where the ignorance of the present day wilfully excludes the sun. Suppose the very common case of a family dwelling in London, or in any other of our large towns, and going off in August to the sea-side for a fortnight, or perhaps for a longer time. One of their most pressing cares on setting off is to close every shutter and blind in the entire house from basement to attic. That this step should be taken with windows accessible from the street is not to be wondered at, but why rooms on the second floor and the attics should thus be consigned to darkness for such a lapse of time it is hard to understand. Perhaps to leave any opening for the light to enter would be a social sin for which Mrs. Grundy has in reserve heavy penalties. We lift up our eyes, and, looking from our library window across two series of suburban gardens, in which

> "Life dies, death lives, and Nature breeds
> Perverse all monstrous, all detested things,"

we see the back of a house which has thus been closed for

a month. The inhabitants have even fitted their little conservatory with blinds, striped pink and white, and the unhappy plants are all this time enjoying the luxuries of etiolation. Now when these good people return home, will not the air of a house which has been been kept so long without ventilation and insolation more than counterbalance any benefit which they may have received from the sea-breezes and the problematical ozone of Bournemouth or Torquay? We know an instunce where a family, on thus returning to their shut-up dwelling, were attacked with what was called a "low fever," which, they maintained, had been caught at the watering-place where they had sojourned. To convince them of the truth was impossible.

V. PHYSIOLOGY AND ITS OPPONENTS.*

SCIENTIFIC research, and even the study of science altogether, have been opposed and prescribed at different times under varied pretexts. At one time they were condemned as impious, at another as ungentlemanly in comparison with "classics," and now one at least of the main branches of biological science is branded as cruelty, with all the most ignominious epithets which the heated imagination of neuropaths or the rancour of professional agitators can supply. Some persons may indeed plead that the wrath of the Bestiarians or Humanasters is directed not against physiology in itself but against experiments upon living animals. We reply in the words of Shakspeare:—

> "You take my life
> When you do take the means whereby I live."

In like manner you paralyse a science if you rob it of any of the means of research, of which experiment, the systematic cross-examination of Nature, is, wherever practicable, the most important.

* Apologia or explanation of my last Article on Vivisection which appeared in a London Journal, September, 1, 1884. By a "WEAK WOMAN" who withholds her name.

The Ethics of Vivisection. By J. ROBERTSON. From "Our Corner." August 1, 1885.

We feel it almost our duty to apologise to Mr. Robertson for having bracketted his thoughtful and acute essay along with the hysterical outbreak of the anonymous "Weak Woman." Both, however, like many other writers on either side of the question, commit one oversight. They seem to conceive of physiological experimentation as a purely *medical* question, the results of which are or ought to be directly applicable to medicine, or to veterinary practice. This error renders it possible to raise the question whether any particular improvement in medical or in surgical treatment is due to such experiments? But what would, for instance, be the position of chemistry or of physics if every train of research had to be justified on account of its direct, immediate utility in medicine or even in the arts? It is now everywhere conceded that progress in the arts requires not merely a narrow knowledge of some few substances and their reactions, but a general abstract knowledge of the whole sphere of phenomena concerned. It has been seen that theoretical, speculative researches on the derivatives of coal-tar have led to a new and a lucrative industry. Behind medical practice stand chemistry and physics developed without any direct reference to the art of healing. Why, then, should the case be other with physiology? Just as the physician regulates his practice by an insight into the chemical constitution and behaviour of the various substances which form the human body, and of the foods, drugs, poisons, and disease germs which are intentionally or accidentally introduced into it, so, and even in a higher degree, he requires a general knowledge of animal life, as accurate and as profound as can be attained, and for this purpose physiological experimentation is necessary.

Mr. Robertson notes that the Bestiarians "strongly contend that physiological experiment is morally indefensible even if serviceable." The steps in the controversy he considers as few and simple. The Bestiarian alleges that man has "no right" to give animals pain with the view of escaping it himself, or as it would be more properly put in pursuit of knowledge not otherwise attainable. The physiologist "alleging that the right does exist puts it on all fours with the right to use animals for food or to make them work. The Bestiarian replies that, granted the right to kill for food and to put to service, the right to give pain does not follow. And here sets in the dead lock." Now with all respect for Mr. Robertson we can see here no dead lock at all, the last contention of the Bestiarian being in our judgment unmitigated nonsense. We cannot drive animals to market,

thence to the slaughter-house and then put them to death with the pole-axe or the knife without giving them pain. We cannot deprive animals of their natural freedom, castrate the major portion of them (a decided piece of vivisection, as Mr. Robertson points out), and force them to work all their lives under penalty of the whip without giving them pain. If, therefore, we have the right to kill and to compel to work we have the right to give pain. And it is undeniable that the pain inflicted in these two ways, could it be arithmetically summed up, would be found incomparably greater than that which has been and is being inflicted in all the physiological laboratories of the world. Those who believe the sensational *historiettes* about "carving to death in cold blood" should recollect that even putting fish into pure water, or into water of any particular degree of hardness with the view of stuying the result is a "painful experiment" within the meaning of the infamous Vivisection Act, and can in Britain be performed only under license!

Mr. Robertson enters upon an enquiry into "rights." He says that the Bestiarian "does not deny the right to put animals to service; and as a rule he concedes the right to kill for food. Now either of these cases will suffice to show that the 'right' of man over animals is simply the assertion of his power. It is occasionally argued, indeed, that where man feeds and cares for an animal his right to its service is one of *quid pro quo*; but it is only necessary to turn to the case of human slavery to find that 'right' in this connexion has no recognition in contemporary morals. No ethical authority now alleges that anyone has the right to keep another in slavery on the score that he gives him food and shelter." . . "We attribute natural rights to those whom we conceive as having the same kind of affections and instincts as ourselves. . . But how, on this view, does it stand with the claim of 'rights' for the lower animals? If we recognised rights inhering in them as in the inferior (human) races, it is clear we should not even feel ourselves entitled to keep them in forced service, much less to kill them; whereas, in point of fact, we do not even trouble ourselves to make sure that we have the 'right' to tame and work horses. The one check we (*i.e.*, men of normal temperament) recognise in the matter is in regard to over-working or ill-using them: there we have scruples; but that only amounts to saying that at a certain point we so far conceive similarity between us and the horse as to shrink from doing him serious injury. . . The 'right' to kill for food, again, can hardly be said to be seriously disputed, for even the extreme vegetarian would

kill animals rather than let them overrun our fields; and it comes to be a question between breeding for slaughter and extermination on the other. . . All round, then, we make our own needs and interests our guides in dealing with animals, recognising no indefeasible right on their side because not holding that they feel as a breach of 'natural right' what would be a breach of that felt by a man. We forcibly tame and burden, we castrate (*i.e.*, vivisect), we kill by forced over-exertion, we kill for food: on what grounds then shall we deny ourselves the 'right' to give animals pain with the object of furthering knowledge?"

This is the inconsistency argument very forcibly put,—the argument out of whose meshes Bestiarians have never succeeded in wriggling. Hence Mr. Robertson is fully justified, as it appears to us, in writing:—"I am not aware of any argument on the (Bestiarian) side which can be pretended to meet that question, and their case, therefore, seems to me to break down as far as the appeal to our sense of 'right' goes.

He next undertakes the difficult task of enquiring into the origin of the Bestiarian fallacy which he has just dissected. The question of the origin of Bestarianism in general is about as difficult as, and is closely analogous to, the question of the origin of small-pox, syphilis, or leprosy. We cannot point out the social "microbe" who first originated this epidemic of irrationality. But upon the state of mind of some of the persons infected Mr. Robertson throws clear "cross lights." He quotes from a pamphlet written by "that wrathful controversialist Miss Frances Power Cobbe," the following characteristic passage:—

"The vivisection of dull (*sic!*) reptiles and wild rats and rabbits wherewith the elder generation of students contented themselves is not alone in question, nor even that of heavy beasts in our pastures; but by some strange and sinister fatality the chosen victims at present are the most intelligent and friendly of our domestic favourites—the cats who purr in love and confidence as they sit beside us on the hearth, the dogs whose faithful hearts glow with an affection for us truer and fonder than we may easily find in any human breast, &c., &c."

It must of course be remembered that there are here misstatements not a few; rabbits, guinea-pigs, and frogs are undoubtedly more frequently selected for physiological experiments than cats and dogs. "Heavy beasts in our pastures" are too expensive and too bulky. It may also be asked whether the purr of the cat is an expression of "love

and confidence" or of satisfaction over comfortable quarters; whether the faithfulness and affection of the pet dog to its owner is not compensated by its spite against all other beings, human or infra-human, which it is capable of injuring or at least annoying.

But amidst all the Victoria Street cheers which Miss Cobbe's fervid periods may call forth let us listen to the calm, logical comments of Mr. Robertson. "All this," says he, "is transparent enough to those ruthless natures which are capable of exploring the quivering frame of this passage with the forceps and scalpel of dispassionate analysis. Miss Cobbe is a typical anti-vivisectionist in that she does what we have said the majority (of mankind) do not do—carry her sympathy with animals to the extent of attributing to them —or some of them—a moral and emotional nature closely resembling our own. She lets it appear very plainly that she is moved by purely personal sentiment as distinguished from an impartial sense of justice. Her dog and cat are a great deal to her, and it is the idea of their suffering which excites her. The exigencies of propaganda force her to be so far consistent as to protest against all vivisection and to deprecate 'sport;' but it is tolerably plain that if it were only dull reptiles, and wild rats and rabbits, and the heavy beasts of our pastures that were vivisected she would not be greatly concerned. That is, she is not defending a 'right' inherent in sentient things as such; *she is doing special pleading for some of them for which* she has a special liking. Her contention virtually is that cats and dogs have a better right to be spared than rats and rabbits, because they have the greater capacity for attachment to [some of] us. Now we have seen that we admit the 'rights" of men because we assume them to feel morally and physically as we should do in their place; but Miss Cobbe's argument to be relevant from that point of view would have to allege that we have cause to believe that dogs and cats under vivisection will feel both morally and physically as we do. We are entitled to believe, on the contrary, that their sensitiveness to pain is less, and that they have no moral feelings analogous to what a man's would be under the circumstances."

These sentences call for a few remarks. Dog-worship, or cynolatry as our contributor, Frank Fernseed, calls it, is the very backbone of the Bestiarian movement. Strange that an animal which owes its popularity mainly to the fact that it is willing to be the accomplice of the human species in cruelty—not to speak of another vice—should play such a part!

That the lower animals do not feel pain as do we is no mere assumption. Mr. E. Gurney wrote in the "Fortnightly Review" for December, 1881 :—"Dogs kept at physiological laboratories for the purpose of experiments display no such horror of the operating-room as was described in a sensational paragraph sent the round of the papers some time ago. A dog released from experiment has been known to jump up upon the operating-table and sit there to inspect a companion having his turn, showing that no very painful impression could have been left upon his mind by that he had undergone." The following incident, taken from "Physiological Cruelty, or Fact *v.* Fancy," * is vouched for by an eye-witness :—

"A house-dog met with an accident by which a large piece of the skin and flesh above the eye-brow was cut and hung loose over the eye. His master, a surgeon, determined to stitch it. Now it is well known that—the skin being exceedingly sensitive—stitching is one of the most painful parts even of serious operations. The dog was taken into a shed, muzzled, and the cut stitched up. All the time it was being done he was straining and struggling to get away, though never whining nor crying. The instant he was released he dashed into a corner of the shed and seized a bone which he had had his eye upon, and which had possessed his soul while he had been undergoing the operation without anæsthetics."

Such cases, to which many more might be added, prove that even such comparatively highly developed animals as dogs are far less sensitive than is civilised man. It is doubtless true that every animal is sentient, and we should be very reluctant to deny the same attribute to plants. But there is a continuous and gradual fading away of sensitiveness from the higher forms of life to the lower.

(To be continued.)

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* Tinsley Bros.

VI. ON THE SUPPOSED GERMINATING POWERS OF MUMMY WHEAT.

By Henry King-Parks.

FROM time to time the alleged power of Egyptian wheat to preserve its vitality for an indefinite period, so as to produce a plant when sown after the removal of the grains from the mummy cases of the ancient Egyptians, has given rise to much controversy. Several writers have asserted that the growth of mummy wheat in England is an established fact; others that, although the evidence is not conclusive, yet it is not improbable that wheat, under certain conditions, is capable of germination after the lapse of thousands of years; whilst there are those who contend that the vitality of wheat is destroyed after a brief period, and that there is no authentic record of mummy wheat germination. My object is to state facts as I find them, instead of relying upon mere theory or assertions, and to give an impartial review of the *pros* and *cons* of the subject.

Now, first of all, it would, perhaps, be prudent to make some inquiry into the character of the natives of the country in which the mummy cases are procured. To their discredit, be it said, the Arabs are represented by our popular writers upon Egypt as being a deceitful race. They are also in the habit of unrolling the mummies in search of coins, &c. They would thus have an opportunity of introducing grains of modern wheat into the mummy cases, and as it would be to their advantage to invest the mummies with as much interest as possible, it may reasonably be inferred that they would not scruple to deceive purchasers by false representations. This fact would not, of itself, be sufficient to discredit the reputed instances of mummy wheat germination unless supported by more tangible evidence. We will therefore analyse the records of a few of the best known cases of supposed germination to see whether the results justify the conclusion that the grains planted were genuine samples of mummy wheat.

In a little book entitled "Botanical Rambles" the author states that during the process of unrolling a mummy at the British Museum many years ago, a few grains of wheat happened to fall out of the linen folds; these grains were picked up by a gentleman present, who planted them, and that although probably two or three thousand years had

elapsed since they were placed there, yet the seed grew and became a healthy plant, producing stalks, each of which bore from *five to seven heads*. I cannot trace any evidence whether the case bore any external signs of having been tampered with, but assuming that the case appeared to be intact, the statement that the stems produced branching ears is of some importance, as there is absolutely no existing proof that this variety of wheat (*Triticum turgidum*) has any antiquity of origin. All the evidence on this point, as given in my treatise on "Ancient Wheat," * goes to prove that wheat with branching ears was unknown before the time of Pliny. Certainly no drawings of it have been found on the ancient tombs; and although Unger † believes it was cultivated by the ancient Egyptians, he furnishes no proof founded on either drawings or specimens. There is no evidence of the period to which the mummy belonged, as the statement that it was "probably two or three thousand years old" is too vague to base an opinion upon. Wheat with branching ears has been grown in Egypt for very many centuries, and if the date of the mummy under notice was subsequent to the time of Pliny it is probable (if the power of wheat to germinate when thousands of years old *be admitted*) that the case alluded to was a genuine one of mummy wheat germination. It must not, however, be forgotten that this variety of wheat is extensively cultivated in Egypt at the present time; and in the absence of reliable information as to how the mummy case was procured, and having regard to the thieving propensities of the Arabs and their untruthfulness, as well as to the opinions of eminent scientists concerning the vitality of wheat given further on, the case in question would need further corroboration before it can be accepted as authentic.

To the kindness of Mr. William Carruthers, F.R.S., of the British Museum, I am indebted for the knowledge of the next case to which I will refer; and through the courtesy of Mr. John Macgregor, M.A. (Rob Roy), I am favoured with particulars concerning it.‡ Mr. Macgregor's well-known accuracy of detail, and the high character of the gentlemen named, entitles the following to special prominence. On June 9, 1871, a wooden sarcophagus, brought from the Khedive of Egypt by the Duke of Sutherland, was opened at Stafford House by Dr. Birch of the British Museum, and

* Ancient Wheat: Being a History of Wheat and its Products, from Prehistoric to Modern Times. Second (Illustrated) Ed. By HENRY KING-PARKS.

† UNGER. Die Pflanzen des Alten Agyptens, p. 31.

‡ Letter to the writer.

Mr. John Macgregor. The lid of the mummy-case seemed to have been unopened before, as the clay luting was in the joint nearly all round. From the hieroglyphics outside Dr. Birch pronounced the mummy to be that of a priest sacred to Osiris, and about 2600 years old; and he predicted there would be found inside several articles, a blue glass net, images, &c., which were found within. Among the loose *débris* inside the case were four grains of corn, two of these Mr. M. planted a few days afterwards in a small flower-pot filled with sifted mould from the Temple Gardens; the other two grains were sent to Mr. Sowerby, at the Royal Botanic Society, Regent's Park; the pot from the Temple was also sent to Mr. Sowerby, in August, with one of the grains grown up about eight inches high; this was returned in December, 1871, as a stalk four feet long, *with twelve grains of oats growing on it;* the other two grains grew up into several stalks, but without any fruit. In connection with this case it will be noticed that Dr. Birch pronounced the mummy to be 2600 years old, and that the seeds which vegetated produced *oats—a grain unknown to the Egyptians* 2600 *years ago!* This is unmistakable testimony that the Arabs are not only adepts in lying, but skilful manipulators of ancient sarcophagi, and has an important bearing upon the first, and the next case of wheat vitality.

The next, and perhaps most talked of, experiment is that of the Count of Sternberg, at Prague.* I take the particulars from A. de Candolle's learned work on "The Origin of Cultivated Plants." The Count of Sternberg had received the grains from a trustworthy traveller, who assured him that they were taken from a sarcophagus. Two of the seeds germinated, it is said, but M. de Candolle ascertained that in Germany well-informed persons believe there is some imposture, either on the part of the Arabs, who sometimes slip modern wheat into the tombs (even maize, an American plant), or on that of the *employés* of the Count of Sternberg. These three cases, and there are several others of less note, are cited as being good examples of the various experiments with supposed mummy-case seeds.

It is somewhat remarkable that almost all of the published accounts of the experiments are qualified by the statement that owing to the indifferent character of the Arabs it is not always certain that the seeds which have sprouted were really at first enclosed with the mummies. Thus, in "Vegetable Physiology," the author asserts that there is nothing

* Journal Flora, 1835, p. 4.

improbable in the fact of ancient seeds germinating, but yet he gives expression to a doubt, such as the above. A. de Candolle,* too, says that "the thing is not impossible, for grains are all the better preserved that they are protected from the air, and from the variations of temperature or humidity, and certainly these conditions are fulfilled by the Egyptian monuments," but he also says that "*as a matter of fact, the attempts at raising wheat from these ancient seeds have not been successful,*" and, further, that "*no grain taken from an ancient Egyptian sarcophagus, and sown by horticulturists, has ever been known to germinate.*" Dr. F. W. Pavy,† the analyst, says:—"I see no reason that mummy wheat should not grow, as seeds if kept dry undergo no change. If wheat can be kept for twenty years with its capacity under exposure to the requisite conditions—warmth, air, and moisture—for assuming a state of active life, that is, passing from dormant to active vitality, there is no reason that it may not be kept for 100 years; and if for 100 years, no reason that it may not be kept for 1000 years, and upwards. Everything depends upon the conditions under which the grain of wheat is kept—whether antagonistic to change taking place in it or not;" but, adds Dr. Pavy, "*I do not know of any specific instance to refer to.*" The opinion of a gentleman occupying Dr. Pavy's position is entitled to respect, but it will be seen that the conclusions of the learned author and analyst are based upon theory, and that he has no ascertained facts to support his contention. Thus far, it would seem, 1st, that the best known cases of the supposed growth of ancient seeds of wheat have a considerable amount of suspicion attached to them; and 2nd, that those who assert that the seeds of wheat are capable of retaining their vitality for an almost indefinite period, when preserved under certain conditions, are unable to quote an authenticated instance of germination.

As against these conjectures I will quote the opinions of our modern authors and scientists.

Mr. Villiers Stuart, M.P., an old traveller in Egypt, and author of "Nile Gleanings," writes thus:‡—"With regard to the passage about Egyptian Wheat, I beg to say there is no such thing in existence as mummy wheat that will grow. The vases full of mummy wheat which are sometimes found with mummies are invariably carbonised from age. The samples represented as having vegetated are due to the

* Origin of Cultivated Plants.
† Letter to the writer, January 18, 1884.
‡ Letter to the writer.

deceitfulness of the Arabs, who put modern wheat into the old vases and sell them to travellers. Nothing but a miracle could make mummy wheat germinate."

Mr. William Carruthers, F.R.S., Consulting Botanist to the Royal Agricultural Society, says:*—"The statements in your papers as to mummy wheat are quite correct. I have examined many specimens in the British Museum, and I found every one incapable of germination. I have had a case of supposed germination investigated. Mr. John Macgregor, "Rob Roy," germinated, he believes, two mummy seeds, but the plants that grew were oats—a grain unknown to the Egyptians! A pea, also, was sent to me, said to have been grown from a seed taken out of a mummy-case, but on applying to the person who had taken it out he informed me that he had only the testimony of the Arab from whom he had got the peas. *No* case investigated has turned out to be trustworthy. As a matter of fact, the seeds of wheat are incapable of germination when they are ten or twelve years old."

Professor Sir Charles A. Cameron, M.D., President of the Royal College of Surgeons, Ireland, Chemist to the Royal Agricultural Society, Ireland, M.R.I.A., &c., writes explicitly thus:†—"I never believed that the so-called mummy wheat was really composed of seeds which had been deposited in the tombs of the kings of ancient Egypt at the time of their sepulture. So far as I can learn, it is alleged that the wheat found in the tombs germinated very satisfactorily. Now we know that it is only the best matured seeds of plants which satisfactorily germinate, and every seed-grower is aware that the vitality of seeds is influenced by many factors. The seeds produced under unfavourable climatic influences sometimes are incapable of re-producing their parent plants. Very old seeds are known, too, to be much inferior to those of recent growth. No doubt seeds retain their vitality for a very long period, but when thousands of years old, I am quite sure that the embryo plants which they contain could have no vitality left. One might as well suppose that an egg embalmed for a thousand years would be capable to produce a chick, when hatched, as to believe that organic matter could be associated with dormant vegetable life for a period of nearly forty centuries."

Sir J. B. Lawes, Bart., J.P., LL.D., F.R.S., F.C.S. (who, in conjunction with Dr. J. H. Gilbert), has written in the

* Letter to the writer, February 7, 1884.
† Letter to the writer, January 17, 1884.

Journals of the Royal Agricultural Society of England, several valuable reports on field experiments, &c., upon wheat, including, "On some points in the Composition of Wheat Grain: its Products in the Mill and Bread;" "Report of Experiments on the Growth of Wheat for Twenty Years in succession on the same land;" "On the Home Produce, Imports, and Consumption of Wheat;" "Our Climate and our Wheat Crops;" "Another Paper on the Home Produce, Imports, Consumption, and Price of Wheat, &c.)," writes as follows:*—"I have not the slightest idea that the mummy wheat which was brought to this country a few years ago, and said to be thousands of years old, was anything except the modern wheat of the country. All the evidence we have upon the germination of seeds tends to prove that their power of germinating deteriorates very soon, and starchy seeds much sooner than oil seeds."

The late Dr. Augustus Voelcker, Chemist to the Royal Agricultural Society of England, &c., gave this opinion:†—"My father desires me to tell you that he does not know of a single authenticated case of mummy wheat having germinated after the lapse of so many years, and he regards it altogether as a myth the possibility of such germination ever having taken place. Even under most favourable conditions the vitality of wheat probably will not be preserved for a great number of years."

From an English newspaper I extract the following:‡—"Most people have heard the story of the astonishing vitality of wheat which had been wrapped up with an Egyptian mummy for ever so many thousand years, and a few persons may, perhaps, have been foolish enough to part with their money in the purchase of some grains that were advertised some time ago as 'Mummy Wheat.' The following reply to a question that was put to Sir J. D. Hooker a few days ago may interest them. He writes:—'The story of Egyptian mummy wheat having germinated has never been confirmed, and is not credited by anyone who is warranted by knowledge and experience in such matters to give an opinion. Innumerable attempts to stimulate mummy wheat into vitality have each and all failed.—Your obedient servant, J. D. HOOKER. Royal Gardens, Kew.'"

Further evidence of the same nature is forthcoming, but I consider that I have given sufficient to disprove the statement that the seeds of wheat can retain their vitality for the

* Letter to the writer, January 8, 1884.
† Letter to the writer, from Mr. E. W. Voelcker, January 10, 1884.
‡ *Marlborough Times*, July 11, 1885.

long period which they are popularly represented to have done when taken, as is supposed, from ancient mummies, and afterwards planted.

To sum up, it will be observed that Sir J. B. Lawes, one of our greatest living authorities upon Agricultural Science, declares that the power of germination deteriorates very soon, especially in starchy seeds; that Sir Charles A. Cameron, who has had very great experience as an Agricultural Chemist, asserts that the very old seeds are known to be much inferior to those of recent growth, and when thousands of years old he is *quite sure* the embryo plants which they contain could have no vitality left; that Mr. William Carruthers, head of the Botanical Department of the British Museum, who *examined many specimens in the Museum*, says he found *every one of them* incapable of germination; that the late Dr. Voelcker, Chemist to the R.A.S.E., did not know of *a single authenticated case*, and that he did not believe that the vitality of wheat could, *even under the most favourable conditions*, be preserved for many years; that Sir J. D. Hooker, of the Royal Gardens, Kew, whose valuable contributions to botanical literature have a world-wide fame, asserts that the innumerable attempts to stimulate mummy wheat into vitality have each and all failed; that the seeds found by Mr. John Macgregor, the talented author of "Rob Roy on the Jordan, Red Sea," &c.; in a mummy case, *bearing all the outward appearance of never having been tampered with* after being planted, produced *oats*; that all those writers who believe in the power of ancient wheat to germinate cannot refer to any specific instance, and admit, like A. de Candolle,* that the fradulent introduction of modern plants into the sarcophagi of the mummies is a possible source of error, this author relating an instance of the discovery of grains of maize (a plant of American origin) in one of the mummy cases, and which were introduced by the Arabs; and, lastly, that Mr. Villiers Stuart, M.P., an old traveller in Egypt, and who is intimately acquainted with the people of that country, emphatically states that the Arabs do not hesitate to enclose *modern* wheat in old vases, and such like antiquities.

Sandymount, Dublin.

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* Origin of Cultivated Plants.

VII. THE BRITISH ASSOCIATION AND SIR LYON PLAYFAIR'S PRESIDENTIAL ADDRESS.

THE future of the British Association is not quite unclouded. Its annual gatherings show no falling off in the number of attendance, nor are we aware that any lack of funds is threatened. The daily and weekly papers, provincial as well as metropolitan, still find it desirable to report the proceedings at considerable length. Special sermons are still preached to, or perhaps rather *at*, the Association wheresoever it meets. Last year's gathering at Montreal presents even some of the features of a notable progress, foreshadowing the rise of an Association for the Promotion of Science, which, if not absolutely international, will at least embrace not merely the United Kingdom, but the British Empire and the United States of America,—in other words, the English-speaking nations of the Globe. May it so fall out! But for all this it seems to us, and to not a few others, that a change of ill omen is gradually stealing over the meetings. It would be a delicate and an invidious task to decide whether the work done by the Sections has deteriorated in quality, but it certainly has fallen off in point of quantity, and in the number of hearers.

Eminent names will still draw a numerous audience, but a young *savant* who has not yet won his spurs will certainly not find the sectional meetings of the Association a fit opportunity for bringing his researches before qualified and appreciative listeners. On the other hand, the amusing features of the gathering, excursions, garden-parties, lunches, and what not come more into prominence, and, not content with the evening, are gradually encroaching upon the afternoon. The bread is decreasing to a poor halfpenny-worth, while the sack is becoming an intolerable deal. Hence there are those who—not scientific specialists, but observant men of the world—question whether the Association has in it another half-century's life.

Our immediate concern is not, however, with the general position and prospects of the British Association, but with the Inaugural Address of its President. Sir Lyon Playfair did not give, as some of his predecessors have done, a general picture of the most recent developments of Science, of the direction they are likely to take, and of their bearings on

the various interests of society. He did not enter upon any controversy attacking or defending any contested theories. His subject, that of secondary education, however important, was one of not unquestionable relevance. Further, though many of his statements deserve the most eager and careful attention, he himself seems scarcely to have felt their full import. Had he done so he could scarcely have failed to draw and to emphasize certain conclusions of which we find only the slightest hint.

He points out our deficiencies in secondary education,—deficiencies, perhaps, in no small extent due to the fact that we have preferred to force primary education upon those who do not want it, at an untold cost, and we have a lack of funds for supplying higher education to those who crave for it. He says:—

"Various royal commissions have made inquiries and issued recommendations in regard to our public and endowed schools. The Technical Commission which reported last year can only point to three schools in Great Britain in which Science is fully and adequately taught. While the Commission gives us the consolation that England is still in advance as an industrial nation, it warns us that foreign nations, which were not long ago far behind, are now making more rapid progress than this country, and will soon pass it in the race of competition unless we give increased attention to Science in public education. A few of the large towns—notably Manchester, Bradford, Huddersfield, and Birmingham—are doing so. The working classes are now receiving better instruction in Science than the middle classes. The competition of actual life asserts its own conditions, for the children of the latter find increasing difficulty in obtaining employment. The cause of this lies in the fact that the schools for the middle classes have not yet adapted themselves to the needs of modern life. A return just issued, on the motion of Sir John Lubbock, shows a lamentable deficiency in Science teaching in a great proportion of the endowed schools. While twelve to sixteen hours per week are devoted to Classics, two to three hours are considered ample for Science in a large proportion of the schools. In Scotland there are only six schools in the return which give more than two hours to Science weekly, while in many schools its teaching is wholly omitted. Every other part of the kingdom stands in a better position than Scotland in relation to the science of its endowed schools.

"*As there is no use clamouring for an instrument of more compass and power until we have made up our mind as to the*

tune, Prof. Huxley, in his evidence before a Parliamentary Committee in 1884, has given a time table for grammar schools. He demands that out of their forty hours for public and private study, ten should be given to modern languages and history, eight to arithmetic and mathematics, six to science, and two to geography, thus leaving fourteen hours to the dead languages. The great public schools of England will continue to be the gymnasia for the upper classes, and should devote much of their time to classical and literary culture. Even now they introduce into their curriculum subjects unknown to them when the Royal Commission of 1868 reported, though they still accept Science with timidity. Unfortunately the other schools which educate the middle classes look to the higher public schools as a type to which they should conform, although their functions are so different. It is to the interest of the higher public schools that this difference should be recognised, so that, while they give an all-round education and expand their curriculum by a freer recognition of the value of Science as an educational power in developing the faculties of the upper classes, the schools for the middle classes should adapt themselves to the needs of their existence, and not keep up a slavish imitation of schools with a different function.

"In the school the boy should be aided to discover the class of knowledge that is best suited for his mental capacities, so that, in the university, knowledge may be specialised in order to cultivate the powers of the man to their fullest extent.

"Passing from learned or public professions to commerce, how is it that in our great commercial centres, foreigners—German, Swiss, Dutch, and even Greeks—push aside our English youth, and take the places of profit which belong to them by national inheritance? How is it that in our colonies, like those in South Africa, German enterprise is pushing aside English incapacity? How is it that we find whole branches of manufactures, when they depend on scientific knowledge, passing away from this country, in which they originated, in order to engraft themselves abroad, although their decaying roots remain at home? The answer to these questions is that our systems of education are still too narrow for the increasing struggle of life.

"The opponents of science education allege that it is not adapted for mental development, because scientific facts are often disjointed and exercise only the memory. Those who argue thus do not know what Science is. Scientific and

literary teaching have different methods; for while the teacher of Literature rests on authority and on books for his guidance, the teacher of Science discards authority and depends on facts at first hand, and on the book of Nature for their interpretation. Natural science more and more resolves itself into the teaching of the laboratory. In this way it can be used as a powerful means of quickening observation, and of creating a faculty of induction after the manner of Zadig, the Babylonian described by Voltaire."

Sir Lyon Playfair complains, with the fullest justice, that in our schools and colleges the time and the facilities devoted to the study of Science are insufficient in themselves, and still more insufficient—as he notices in subsequent portions of his Address—in comparison with those of rival nations. But we must call especial attention to the words which we have italicised. The foreign educational systems play a "tune" differing from ours not merely in the time and money expended, but in another point perhaps even more important. *Their* teachers of Science are not paid by so-called "results." Their college curricula do not involve a constant preparation for examinations. The principle of competition plays no prominent figure. They have no merely examining universities. Now, as Sir Lyon Playfair admits our inferiority, it seems strange that he does not weigh, one by one, all the points on which the British and (say) the German systems of education respectively differ, to decide where the blame must fall. Is it not, at any rate, possible that the fault lies here? And, if so, no possible increase of time and expenditure will get over the difficulty. Why is it said with truth that the senior boys in a grammar school learn Science less easily than the juniors? Why are we told that the boys who shine at school disappear at the university, and that the shining lights of the university disappear, in turn, in after life? This is not the case in Germany: the brilliant student—recognised as such by his power not of passing "exams," or of being easily crammed, but by his faculty for conducting research—becomes in after life the still more brilliant *savant*. Perhaps the "English incapacity" of which Sir Lyon Playfair complains has been laboriously instilled into the unfortunate youth at school and college. "All the brains," as an eminent friend of ours says, "have been examined out of him."

The President of the British Association tells us that—"Either all foreign States are strangely deceived in their belief that the competition of the world has become a competition of intellect, or we are marvellously unobservant of

the change which is passing over Europe in the higher education of the people."

Might he not have said, in like manner, that "either all foreign States are strangely deceived in their belief and practice, or we are marvellously astray in maintaining and extending our examinational system." He says most truly:—"Universities are not mere storehouses of knowledge; they are also conservatories for its cultivation. The widening of the bounds of knowledge, literary or scientific, is the crowning glory of university life. Germany unites the functions of teaching and research in the universities, while France keeps them in separate institutions. The former system is best adapted to our habits, but its condition for success is that our science chairs should be greatly increased, so that teachers should not be wholly absorbed in the duties of instruction. Germany subdivides the sciences into various chairs, and gives to the professors special laboratories. It also makes it a condition for the higher honours of a university that the candidates shall give proofs of their ability to make original researches."

"Unless English colleges have such ambition they may be turned into mere mills to grind out material for examinations and competitions. Higher colleges should always hold before their students that knowledge, for its own sake, is the only object worthy of reverence."

Here Sir Lyon Playfair admits much of the truth, but he still falls short of the one final step. He still says "the Department of Science and Art is doing excellent work," and speaks of the "excellent school in South Kensington."

Now the "Department of Science and Art" has been at work for many years, and has been entrusted with the expenditure of a very considerable sum of public money. And what have been the "results"? We shall venture to guage them by an unusual, but we think perfectly fair, standard. Not a few authorities have pointed out, as one of the most alarming symptoms of our national inferiority, the number of aliens who find employment in Britain, the alleged reason being the incompetence of natives. Now has this number at all decreased since the establishment of the Science and Art Department? Has it shown any tendency to decrease? Is it not still increasing? And if such be the fact, what is the only legitimate inference? Suppose a physician has gone on for a long time treating his patient in a certain manner, but finds him, if anything, getting worse: will he still persevere with increased doses, and if he does so will he not be "pushed aside" for incapacity? We fear

that if the higher officials of the Department were paid by results there would be weeping and wailing at South Kensington. But Sir Lyon Playfair does not see all this: we fear he is too deeply committed to the competitive and examinational system. We have, with unspeakable regret, found him connected with the attempt to resist the movement against over-pressure. For one of the characteristics of the English system of education is, that it wastes not merely time and money, but nerve-power and intellect.

If we were asked how to reform this vicious system from top to bottom, we would say—Disband in the first place the Committee of Council on Education and the Science and Art Department, and commit their duties to a Minister of Public Instruction, who must, whatever other qualifications he possesses, be an eminent physician. Do away with payment by (pseudo)-results and with the "standards," and with every other feature which aims at stereotyping human beings. Divide the universities from the very outset into faculties, each student to declare the faculty which he selects. No student to be compelled to take up any subject not included in his faculty. Examinations to be required merely in the case of candidates for professions under State control, such as lawyers and medical men. Degrees, fellowships, and other honours to be earned only by successful research, and not by mere proficiency in producing the results of others. Rise in the professions, such as the Army Medical Service, not by renewed examinations, but *rebus gestis*. Finally, laboratories—physical, chemical, biological—under competent instructors everywhere; preparing for examinations, coaches, and cram-books nowhere. Let these changes be made, and there will be no scope for alien men of Science in Britain.

But why dream of the nation thus coming to its senses? We shall go on establishing more and more examinations, training youth to talk about things in place of doing them, until we take the place in the civilised world which China seems likely to abandon.

ANALYSES OF BOOKS.

On the Sensations of Tone as a Physiological Basis for the Theory of Music. By HERMANN L. F. HELMHOLTZ, M.D., Foreign Member of the Royal Societies of London and Edinburgh. Second English Edition. Translated, thoroughly Revised and Corrected, rendered conformable to the Fourth (last) German Edition of 1877, with numerous additional Notes and a new additional Appendix, bringing down information to 1885, and especially adapted to the use of Musical Students, by A. J. ELLIS, F.R.S., &c. London: Longmans, Green, and Co.

WE have here an extraordinary work, the production of an author who combines attainments rarely met with in one and the same individual. Having been formerly Professor of Physiology in the University of Heidelberg, he now fills, with distinction, the Chair of Physics in that of Berlin. This fact already bespeaks no small degree of versatility, since it is generally found that the physicist and the physiologist belong to two very distinct mental types. But this is by no means all: Professor von Helmholtz is theoretically, if not also practically, a musician. Had he not been such, the work before us could never have been written.

Mr. Ellis, the translator, is also evidently a man of no small degree of versatility. That he is a musician is a matter of course. On the title-page he is described as "twice President of the Philological Society," and "Member of the Mathematical Society." Had he been a specialist, limited to his speciality,—or, as the Americans profanely put it, "a one-horse man,"—he could never have succeeded in producing a satisfactory version of Prof. von Helmholtz's "Ton-Empfindungen."

In his Introduction the author discusses the relations subsisting between physical and physiological acoustics on the one side, and of musical science and æsthetics on the other. These different disciplines have remained hitherto widely apart, and without any special influence the one upon the other. He argues that music stands in a much closer and more immediate connection with pure sensation than in any other of the fine arts. Hence the theory of the sensations of hearing is destined to play a much more important part in musical æsthetics than does, for example, the theory of chiaroscuro, or of perspective in painting. He writes:—"When in hearing a concert we recognise one tone as due to a violin, and another to a clarionet, our artistic enjoyment does not depend upon our conception of a violin or a

clarionet, but solely on our hearing the tones which they produce, whereas the artistic enjoyment resulting from viewing a marble statue does not depend on the white light which it reflects into the eye, but upon the mental image of a beautiful human form which it calls up."

The author further points out the distinction between physical acoustics, which is essentially nothing but a section of the theory of the motions of elastic bodies, and physiological acoustics. He shows that inquiries into the processes of any of our sense-organs have generally three distinct parts. First, we have to discover how the external agent reaches the nerves to be excited. This may be called the physical part of the investigation. Secondly, we have to investigate the various modes in which the nerves themselves are excited, giving rise to their various sensations. And thirdly, the laws according to which these sensations result in mental images of determinate external objects,—that is, in perceptions. Thus we have a physiological investigation for sensations, and a psychological investigation for perceptions. The results obtained for the physiological and psychological sections of the theory of hearing are but few, imperfect and accidental.

The first part of the work before us is essentially physical and physiological, containing a general investigation of the phenomenon of harmonic upper partial tones. The second part treats of the disturbances produced by the simultaneous production of two tones. The third part discusses the construction of musical scales and notes, and consequently falls within the sphere of æsthetics.

A Handbook of Practical Telegraphy. By R. S. Culley. London: Longmans, Green, and Co.

This work, having already reached its eighth edition, can scarcely be regarded as amenable to criticism. All persons who feel an interest in electric telegraphy—which alone falls within the author's plan—will have already formed an opinion upon it, mainly favourable.

The author considers in succession the sources of electricity, the laws of the current, the Earth as part of a circuit, magnetism and electro-magnetism, static and dynamic induction, atmospheric electricity and earth-currents, the construction of telegraphs, testing, signalling, and submarine telegraphy.

We note the remark that "public opinion" will not tolerate the erection of telegraph-poles in the streets of towns. We

should rather be disposed to say "public convenience." If in the carriage-way they and their systems of wires might interfere with traffic; and if in the footway, and consequently near the houses, they might prove serious hindrances to the manipulation of fire escapes, ladders, &c. Indeed, objectionable as wires are when stretched over the tops of houses, they are still more to be deprecated if traversing the streets at lower altitudes. The recommendation that when a street must be crossed it should be at right angles, so as to decrease the span and thus lessen the risk to passengers, by no means meets with general attention. It is undoubtedly true that the number of accidents has hitherto been very small, but it is likely to increase in course of time as the wires get older. In cases of heavy snow-storms and of conflagrations, there is here a very serious risk.

It is curious that the industry of spiders occasions no small annoyance. Their webs often collect dust and moisture, and thus increase the unavoidable loss at the insulators.

A curious trait of human—or at least of juvenile—character is mentioned in connection with insulators. "The dark colour and unpretending appearance of a small ebonite insulator protect it greatly from wilful damage. It is found that in localities where the telegraph is exposed to mischief, a large white porcelain insulator is quickly broken, earthenware is less quickly attacked, and a small ebonite insulator not at all. Mischievous boys cannot hit the small mark with stones, nor does it tempt them by its inviting appearance. Many who would not take the trouble to break an unpretending black cup cannot resist the temptation of throwing at a large white one.

Solutions of the Questions on Magnetism and Electricity set at the Intermediate Science and Preliminary Scientific Pass Examination of the University of London, from 1860 *to* 1884. By F. W. Levander, F.R.A.S., Assistant Master, University College School, London. Second Edition, Corrected and Enlarged. London: H. K. Lewis.

This is a work which does not commend itself to our sympathies. We believe it to be an outcome of a vicious system, with whatever ability it is worked out.

The author states, in his Preface, that "Persons desirous of presenting themselves for Examinations are deterred from doing so by a want of some further information *as to the requirements of the Examiners* than can be gained from a mere syllabus, or from the inspection of a few examination papers."

The words which we have italicised seem to us to convey an admission that in Electricity and Magnetism, and by a parity of reasoning in other branches of Science, a knowledge of the subject-matter is insufficient without an acquaintance with the idiosyncracies of the examiners. In other words, it accentuates the fact that to "know" and to "pass" are not connected together by any essential necessity.

In a laudatory notice of the first edition of this little work, by a medical contemporary, we find also the following significant remark:—"Many students fail from a want of knowledge how to put their answers concisely into words." This is another impeachment of the modern English educational system, all the more telling because strictly unintentional. It is tantamount to a confession that an acquaintance with any subject is insufficient if the student lacks the power of talking about it in a certain approved style.

If we waive, however, these principial objections we find that Mr. Levander has executed his task exceedingly ably. His book will be unquestionably useful and welcome to those—and they are not few—whose tastes or whose necessities make the passing an examination seem to them a desirable object. The treatise is in fact a "coach," consisting of paper and ink instead of flesh and blood.

The Harz Mountains: their Geological Structure and History. By H. M. CADELL, B.Sc., of the Geological Survey of Scotland.

THIS memoir is a reprint from the Proceedings of the Royal Physical Society of Edinburgh. The author points out in the outset that the Harz is, like the Alps, Pyrenees, and Himalayas, a mountain system of elevation, and not, like the Norwegian mountain mass or the Scottish Highlands, a table-land of denudation. Its culminating point, the Brocken, of uncanny memory, is 3746 feet in height, but is supposed to have been at one time much more elevated. The area now occupied by these mountains is described as having been during the Palæozoic period a portion of the sea, in which the Silurian, Devonian, and Lower Carboniferous formations of Central Europe were deposited. The rocks with their fossils show changes in the depth and condition of this sea, but they form throughout a perfectly conformable system. In the Devonian period there was vigorous volcanic action, with protrusion of diabases and tuffs. About the close of the Lower Carboniferous the sea became shallower, the rocks which had been formed below the sea were upheaved

in a series of folds trending generally from N.E. to S.W. Next followed the irruption of great bosses of granite; after which the region was dislocated by powerful faults. The fissures thus produced have since been filled with the metallic veins of the Upper Harz. About the end of the Lower Permian the volcanic action ceased, and submergence began. During the Cretaceous the Mesozoic rocks were rent by a great fault in the direction from E.S.E. to W.N.W. On the southern side of this fault the rocky mass of the Harz was upheaved to its present height. Since that date the Harz has not been again submerged, but it has been apparently covered—in part at least—by the great ice-sheet during the greatest intensity of the Glacial epoch.

The work is illustrated with a geological sketch-map of the district and a series of sections, and is enriched with a bibliography.

Geological and Natural History Survey and Museum of Canada. A. R. C. Selwyn, F.R.S., F.G.S., Director. Report o Progress, 1882, '83, and '84.

This bulky volume comprises, in addition to summary reports on the operations of 1883 and 1884, a report on the Geology of the country near parallel 49° N., west of the Rocky Mountains, from the observations made by H. Bauerman; a report on the region in the vicinity of the Bow and Belly Rivers, in the North-West Territory; report on the basin of the Athabasca River; geological observations in the Saguenay region; observations on the Labrador coast, Hudson's Straits, and Bay; explorations and surveys on the Gaspe Peninsula and Prince Edward Island, and a continuation of the survey of the Gaspe Peninsula; explorations and surveys of York and Carleton counties, New Brunswick; Geology of Northern Cape Breton; Apatite deposits of Ottawa County, Quebec; gold-mines of Lake of the Woods; observations on mines and minerals in Ontario, Quebec, and Nova Scotia; analysis of coals and lignites of the North-Western Territory; and contributions from the Laboratory of the Survey.

The volume has two practical defects: it has no Index, and each of the "Reports" of which it consists is paged separately.

Catalogue of Canadian Plants. Part II., Gamopetalæ. By JOHN MACOUN, F.L.S., F.R.C.S., Botanist to the Geological and Natural History Survey of Canada. Montreal: Dawson Bros.

THIS Catalogue will, when completed, prove an exceedingly valuable document. It is to be noticed how many European species—and always, as far as we have noticed, such as are least desirable—have not only become naturalised in Canada, but accompany the settler wherever he has penetrated. Concerning certain plants a doubt may prevail whether they are truly indigenous or have been imported.

The flora of British Columbia seems to approach that of Europe more nearly than does that of the more eastern parts of the Dominion. Thus *Vaccinium myrtillus*, for which the English names whortleberry and bilberry are given as synonyms, is peculiar to the Rocky Mountains, and thence onwards to Alaska. Over all the northern half of Europe it is well known to be common. *Calluna vulgaris*, the common European heather, seems to be found only in the extreme east of the Dominion, and there in but few localities, as if introduced from Europe. No true *Erica* has been found. *Ledum palustre* (marsh rosemary of Europe, Labrador tea of America) seems as abundant in the Nearctic as in the Palæarctic region. *Primula vulgaris* and *P. officinalis* are spoken of as "well-established," both in Vancouver Island and the latter at Cape Breton.

Rich scope for confusion has been given by the practice, common in all colonies, of giving the trivial names of European plants to native species. Of course they may be distinguished by the botanical names, but these are known only to a minority.

The Geological and Natural History Survey of Minnesota. By N. H. WINCHELL, State Geologist. First Annual Report, 1872; Tenth Annual Report, 1881; and Eleventh Annual Report, 1882.

THE first of these reports begins appropriately enough with a notice of earlier explorations, topographical and geological, in the regions which have now become the State of Minnesota. Then follows a summary of the general principles of geology. The elevations of the country above the respective levels of Lake Superior and the ocean are taken chiefly from railway-surveys. This, we think, is scarcely legitimate, since railways as far as possible avoid the more elevated regions.

We find here an analysis of the water of Lake Superior, which is of remarkable purity. The total weight of solid matter in solution is only 2·66 grains per gallon, no organic matter being mentioned as present, though it is added that the permanganate test showed 0·35 part of oxygen consumed by organic matter per million parts of water. The hardness is only 3·5 degrees, and phosphates are absent.

Judging from the three parts before us, Natural History plays no important part in the Survey. We find no account of the flora of Minnesota, and in Zoology there are merely three papers on Crustacea,—*viz.*, the Cyclopidæ of Minnesota, with notes on other Copepoda; the Cladocera of Minnesota; and on Notodromas and Cambarus.

In the Tenth Report there is complaint concerning the frequency and destructive character of forest fires. Special legislation is recommended, and joint action with the Dominion Government is suggested, since the fires begin on both sides of the boundary.

Facts proving that Lightning is a Composite Force. By W. Boggett. London: Ridgway.

In this pamphlet the author adheres to a view which he has expressed in a former publication, *viz.*, that water does not consist of oxygen and hydrogen alone, but likewise of electricity. This hypothesis seems to us to involve the materiality of electricity He does not, however, say that he has experimentally proved this proposition. He says—"Some few, indeed, have been told that it is composed of two explosive gases, but very few trouble themselves to ask how such gases (if water contained nothing else) could possibly extinguish fires, or how they came to be fluid." Now water is not composed of two explosive gases; neither oxygen nor hydrogen is, *per se*, explosive. If a light is applied to them when mixed they burn with an explosion.

The author does not show how or why the presence of electricity as a constituent should enable water to extinguish fires, or render it fluid. He tells us that Faraday proved that ice is a non-conductor of electricity. *Dry* ice is certainly a very bad conductor, though there are not a few substances which conduct electricity still worse. But when melting it conducts *better* than water. Mr. Boggett is exercised by the fact that water when freezing exerts great force if confined,—that is, in a vessel which gives no room for expansion. But what objection can he make to the common explanation that water in freezing assumes a crystalline form, and being thus expanded becomes specifically lighter,—just as solid tin floats upon melted tin? But there

is no need to reproduce arguments which have been already used in dealing with the author's former pamphlet, and which, though expressed without any discourtesy, drew from him not experimental proof, but merely an angry letter. The moment he can give in favour of his theories any better evidence than declarations of inconceivability, we shall not be slow to acknowledge his merits.

Bulletin of the United States Geological Survey. Nos. 2 to 6. Washington: Government Printing-Office.

THE second number consists merely of "gold and silver conversion tables," showing the coining values of ounces of fine metal.

No. 3 describes the fossil faunæ of the Upper Devonian along the meridian of 76° 30' from Tompkins County, Pennsylvania, to Bradford County, New York, by H. S. Williams. The author makes some useful remarks on the study of fossil faunæ. He says:—"We find that in the present ocean the depth of water, the temperature, the degree of saltness, and the freedom from organic impurities, have all a marked influence upon the normal fauna of every part of the ocean. Further, we find that on the two sides of an ocean, on opposite coasts of a continent, and even along a continuous coast-line, in two regions separated by a few degrees of latitude, the faunæ are characteristically different; and when these geographical areas are far separated, although under similar conditions otherwise, the faunæ may contain scarcely a single species in common. The revelations made by deep-sea dredging are also very suggestive. They have shown us that, with all these differences in the upper faunas in the depths of the ocean, species may range from one quarter of the globe to the opposite quarter with scarcely a varietal modification, and when the physical conditions have been continuous geological ages are not too long for the perpetuation of species without important modification. The wonderful effects produced by ocean currents upon the distribution of faunas is brought forcibly to our attention by the reports of our Fish Commission. Their investigations reveal the existence of an abundant fauna within a hundred miles of the New England coast, entirely distinct from the fauna prevailing all along that part of the coast out to deep water; and further, this unique fauna, which at the time of its discovery was composed of a large number of species and a great abundance of individuals, has now apparently left the region altogether. From this we learn that in a very small

geographical area we might find the remains of two entirely distinct faunas, preserved in two strata of a continuous formation lying the one immediately upon the other conformably, yet geologically entirely contemporaneous."

No. 4, by Dr. C. A. White, gives a description of certain aberrant forms of the Chamidæ, from the Cretaceous rocks of Texas; also of a collection of Mesozoic fossils from Alaska, and an account of the Nautiloid genus *Enclimatoceras*.

No. 5 is a dictionary of altitudes in the United States, very useful for reference; and No. 6 is a table of elevations in the Dominion.

Our Corner. A Monthly Magazine of Fiction, Poetry, Politics, Science, Art, Literature. Vol. VI., No. 3. September 1, 1885. London: Freethought Publishing Company.

Mr. J. Horner continues his ably-written instructions for amateur microscopists. In this chapter he treats of solutions and mounting media.

The "Luminiferous Ether," by Laurence Small, is a good exposition of the views held by modern scientific leaders, and will be regarded as an abomination by the "Substantialists," the men who have arisen to set everyone and everything to rights.

From a contemporary is quoted the strange proposal to improve the weather of Western Europe by means of a floating breakwater, moored, we suppose in Davis's Straits. Some years ago it was proposed in this "Journal" to block up these Straits by blasting the rocks on both shores, and causing them to fall into the sea.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

ATTACKS OF THE COMMON GREEN APHIS.

An observation made by the Editor on page 558 of the "Journal of Science," on the possibility of the common green aphis attacking man, is confirmed by my own experience. In my garden is a large elm, with a seat around it. I commonly sit on this to read in the shade. During the dry period which prevailed in July and August I have been ignominiously driven from this position by the Aphides which swarmed in the tree. Their bite produced an irritation corresponding with that described, and the backs of my hands were especially visited. They are very pretty creatures in this winged stage, but by no means loveable. Since the recent rains I am able to return to my favourite seat.

W. Mattieu Williams.

Stonebridge Park, September 5, 1885.

A SEA-SERPENT.

The following paragraph appears to be going the round of the papers:—

"The Quebec ship *Emblem* (Capt. Roberts), from Rangoon, arrived at Queenstown on Sunday morning. The captain states that on August 5th, at 10 o'clock in the morning, and when in lat. 29·35° N., and long. 34·60° W., the mate drew his attention to a sea-serpent which was lying on the surface of the water about 9 feet from the ship's side. It measured no less than 55 feet in length, and about 7 feet in circumference. It was of a light pink colour, and during the time (fifteen minutes) that the crew saw it, its head, which was as big as that of a calf, was slightly raised out of the sea. Several times its mouth opened, disclosing fangs about 4 inches in length. When first seen by the mate it was lying quite motionless, but the water from the

bow of the ship startled it. The weather was good, with a slight wind blowing, the *Emblem* scarcely making four knots."

Whilst holding that the existence of large marine serpents is simply a question of evidence, and cannot be pronounced impossible on *à priori* grounds, I find the above narrative not quite free from suspicion. A head only the size of that of a calf would be very ill adapted for fangs "about 4 inches in length."

SERPENT-HUNTER.

THE EXTERMINATION OF INSECTS.

IN your recent article, "A Glance at Epping Forest," I suspect that you are a shade too severe on the Conservators. Their doings have not been always judicious from the Naturalist's point of view; but the drainage of the Forest cannot have been the sole cause of the disappearance of certain Lepidoptera, since a similar extirpation is going on at other places within an easy distance of London. Thus a few years ago *Zygæna filipendulæ* was not uncommon about Edmonton. *Euchelia jacobeæ* has disappeared from some of its former haunts. Darenth Wood, formerly the Mecca of collectors, is now often "drawn blank." I suspect the main cause of the mischief is the number of boys who have been led to take up not Entomology, but insect-hunting, by the perusal of a certain cheap natural-history paper.

ZETA.

ARE SPIDERS SHORTSIGHTED?

FROM the fact that a spider, especially an *Epeira*, detects the exact locality of an insect caught in its web by applying its feet in succession to the radial lines, it has been inferred that the creatures must be very shortsighted. A fact observed this morning makes me doubt this view. In a neighbour's garden a line supported on posts had been put up yesterday, for the purpose of drying linen, &c., and had been left out all night. On looking over some trees trained against the wall of my garden, and distant about 5 feet from the line, I saw that several Epeiras had carried their webs from the tops of these trees, and from the wall itself to the line. If spiders are shortsighted, how did they become informed of the existence of the line at a convenient distance for their spinning operations?

A SUBURBAN.

DISCOVERERS AND TEACHERS.

In Sir Lyon Playfair's Presidential Address to the meeting of the British Association I find a very questionable assertion. The speaker declared that "In England our discoverers have generally been teachers," and added "I recollect only three notable examples of men who were not—Boyle, Cavendish, and Joule." It seems that he must have forgotten the names of Darwin and Wallace, of Lyell, Murchison, Bates, Belt, Sorby, Pennant, Waterton, White, Crookes, De la Rue, and, I believe, Huggins, Robert Brown, and the two Hookers. These names come into my mind without any searching for, but it is my belief that if the lives of our chief men of Science were collated, it would be found that only a minority of them occupied professorial chairs.

VERITAS.

NOTES.

THE cost of the Laboratories attached to the University of Strassburg has been £211,900.

M. Thollon has laid before the Academy of Sciences a new design of the solar spectrum. The upper band, No. 1, gives the aspect of the spectrum when the Sun is at 80° from the Zenith, and for a mean hygrometric state of the atmosphere. No 2 represents the spectrum when the Sun is at 60° from the Zenith and the air is very moist. No. 3 corresponds to the same distance from the Zenith and to a very dry atmosphere. No. 4 contains the prolongation of all rays of exclusively solar origin, as they would be seen outside the atmosphere.

From correspondence in "Science" it appears that the exact nature of the American Silurian scorpion is still in dispute.

Mr. E. L. Nichols ("American Journal of Sciences") has been experimenting on the sensitiveness of the eye for low degrees of saturation of colours, and among other important results finds that the male eye is more sensitive to red, yellow, and green, whilst the eye of the female is more sensitive to blue.

The custom of preaching *at* the British Association has been duly observed at Aberdeen. The Rev. Prof. Candlish was one of the orators who came forward.

The representatives of the Press complain of a want of courtesy at the Aberdeen Meeting.

Dr. D. Sharp, in a paper recently read before the Philadelphia Academy, seeks to trace the development of the vertebrate eye from the simplest deposit of pigment in an epithelial cell.

Mr. C. H. Murray ("Science") brings home another sin to mosquitoes. He has observed them attacking and destroying newly-hatched trout.

Mr. J. J. Colman and Prof. McKendrick find it impossible to permanently sterilise animal matter by exposure to the low temperatures now at the command of physicists. The microbia survive the cold. Even a frog has been found to recover after having been frozen solid by exposure for half an hour to temperatures of $-20°$ to $-30°$ F.

According to M. Oustalet the fauna of M. Comoro proves that this island has never had a land connection with Madagascar.

Mr. F. M. Slack ("Science"), writing on colour association with months, mentions a relative of his who connects June with green, October with light crimson, and December with blue.

A. Becker ("Mineral. Mittheilungen") maintains, after a prolonged series of experiments, that calcium carbonate in its various natural forms—chalk, arragonite, calcite, &c.—may be converted by heat into a granular limestone, but (at least under the conditions of the experiment) cannot be liquefied, or even softened.

The Lick Observatory, from the extraordinary clearness of the surrounding atmosphere, will enable instruments of the highest magnifying power to be successfully used.

According to Dr. Carl Seiler some persons have a defect of smell analogous to colour-blindness. One person finds that to him violets smell like garlic, everything else smelling normally.

Mr. J. Starkie Gardner, F.G.S. ("Geological Mag."), discusses the possibility of utilising subterranean heat for industrial and domestic purposes.

In a paper on Siberia, the late Dr. Alfred Brehm makes the startling assertion that "The mean temperature is not high enough perfectly to ripen grapes, but oranges grow well in the southern parts." This statement must be received with caution, considering that the orange tree requires a higher temperature than the vine, and is nowhere found able to live and fruit in the open air so far north as 50°.

A specimen of the anaconda (*Eunectes murinus*), 6 metres in length, is now to be seen at the Jardin des Plantes, at Paris.

In refutation of the common opinion that parrots attach no meaning to the words which they utter, we give the following fact:—A child in a neighbouring garden having set up a loud and very cat-like scream, our favourite parrot, a West Coast grey, at once exclaimed "Puss, puss, puss!"

"Cosmos les Mondes" asserts that the chief motive for cremation is hostility to Catholic tradition.

According to C. H. Merriam ("American Naturalist") the Oregon mole, *Scapanus Townsendi*), especially after being soaked in alcohol, displays iridescent colours—purple, lilac, and bronze.

At Rome the proportion of the sexes is very different from that prevailing in England: there are 100 men to 95 women.

In the beginning of last month an exceedingly fine and fresh specimen of *Sphinx convolvuli* was captured in Hornsey Road, by a man employed at the Brush Works. It is now in the possession of Mr. J. Mutch, chemist, of Hornsey Road.

Mr. J. T. Campbell ("American Naturalist") refutes by observation the common notion that the rings of trees register fruitful and unfruitful years.

A notion prevails in Brazil that the *gitiranaboia* (*Fulgora lanternaria*), a perfectly harmless insect, has the power of killing large animals, and even trees, with the touch of its beak.

Taking a ramble along the banks of the Lea, just below the Tottenham Sewage Works, we were saluted by two rough-looking youths with the questions—"Come down to smell at it?" "Aint it strong?"

The teeth of recent animals contain very little fluorine, whilst it is largely present in fossil teeth.

To test the now prevalent opinion concerning the origin of cholera, M. Rochefontaine took a certain quantity of the dejections of a cholera patient containing the celebrated comma-bacillus, and made them into a bolus, which he swallowed. He was not seized with cholera.

A revival of Astrology seems in contemplation. Would it not be possible, as *experimentum crucis*, to find in London two children born at the same time and within a distance of a few yards only, and to compare their destinies in life?

Luiz de Andrade Corvo, in a communication to the Academy of Sciences, contends that the destruction of vines ascribed to the *Phylloxera* is really due to a tubercular disease occasioned by a special bacillus. This disease can be communicated to other plants by inoculation in the entire absence of the *Phylloxera*. This insect merely spreads the evil of inoculation.

Mr. K. A. Chipman ("Science") mentions a case of three children who associated colours with the names of persons, Kate seeming to them red, Mary white, Alice violet, Dick deep Vandyke brown, William a watery blue.

At the September meeting of the Entomological Society, Mr. Waterhouse, of the British Museum, exhibited some aquatic Curculios which swim with the same action and the same ease as a *Dytiscus*, though their legs display no structural adaptation for the purpose.

A certain writer asks—Supposing it asserted that the Moon is made of green cheese, could our *savants* furnish any absolute refutation of the assertion?

A curious poisoning case has come under our notice: a cat which had obtained possession of a sole, and eaten it, became almost immediately very sick, and died before night. The fish had just been bought for human consumption, and the cat—as it may be fairly inferred from her good appetite—was in average health.

The so-called "harvest-man," or "harvest-spider" (*Phalangium*), has been this year very numerous upon kidney-beans infested with black Aphides. Whether it is making war upon this pest, or whether it feloniously attacks the ladybirds, also numerous, is undecided.

The German Congress of Naturalists and Physicians is composed of members and associates, the former alone having the right of voting. Only persons who have written books on some department of natural science, or medicine, are admissible as members. This arrangement, if carried out literally, will of course admit mere compilers.

From the recent proceedings at Southport it is plain that a large proportion, at least, of the working classes are not in favour of the Sunday opening of museums.

Prof. Chrystal, in his Address to the Mathematical and Physical Section of the British Association, admits that "competitive examinations work all this mischief in our educational systems," and speaks of the opportunity of "dethroning the iron tyrant Examination."

According to the "Electrical Engineer," "that vaunted home of education at South Kensington is a hot-bed of something of a very different name, and money that ought to be expended in gaining information for public use goes in directions known to but very few besides those in whose direction it does go."

M. G. Bigourdan ("Comptes Rendus") announces that until lately the nebula of Andromeda had a nucleus comparable to a star of the 10th or 11th magnitude. Almost in place of this small body, but distinct from it, there is now a fine star of the 7th magnitude.

Tanquin, the ordeal poison of Madagascar, has been examined by C. E. Quinquand. It affects in the first place the central nervous system, augmenting especially the bulbo-spinal reflectivity. The animal dies by arrest of respiration, the heart continuing to beat.

Prof. J. P. Lesley, in his Presidential Address at the late meeting of the American Association, says, rather strangely: "Let us avoid the sacrifice of character to Science."

The same *savant* said—"The idea of universal education is wholly modern." What of ancient Athens?

W. W., writing in the "Kansas City Review," shows that in industrial art "the ancients" approached us far more nearly than is commonly believed, and even outdid us in some very important respects.

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